

THE BRITISH PSYCHOLOGICAL SOCIETY

By BEATRICE EDGELL

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1. EARLY YEARS, 1901-1914

Once upon a time it was the accepted convention among historians that their chronicle should be divided into lengths by the reigns of kings or queens. Though this chronicle cannot follow out that neat practice, it does at least begin decently and in order in the first year of the reign of Edward VII. On 24 October 1901, ten persons met together at University College, Gower Street, London to form a psychological society on the same lines as those followed by the Physiological Society. These ten persons were: Prof. James Sully, Grote Professor of Mind and Logic at University College, by whose invitation the meeting had been summoned, Dr Sophie Bryant, Headmistress of the famous North London Collegiate School, Mr Boyce Gibson, Lecturer on Philosophy and Psychology at Westfield College and also at Hackney College and New College, Mr F. N. Hales, a Scholar of Cambridge, Dr Robert Armstrong Jones, Senior Resident Physician at the London County Council Asylum at Claybury, Dr W. McDougall, Fellow of St John's College, Cambridge, and a student under Dr Rivers, and co-operating with Prof. Sully at University College by holding a weekly class in experimental psychology, Dr F. W. Mott,¹ Pathologist at Claybury and Professor of Physiology at Charing Cross Hospital Medical School, Dr W. H. R. Rivers, Lecturer in Experimental Psychology and the Physiology of the Special Senses at Cambridge, Dr W. G. Smith, Lecturer in Psychology at King's College, London, and Mr A. F. Shand, a moral science graduate of Cambridge, interested in psychology for its own sake.

From this catalogue one can see that the ten persons who met together to found the society represent in themselves just the men and women who make up the society to-day: the psychological lecturer and laboratory worker, the educationist, the physiologist, the psychiatrist and the private individual interested in things of the mind.

What were the stirrings of spirit which led to this meeting and what was the character of psychology at this date?

The most outstanding feature of psychology was the development of experimental and of quantitative methods. In Germany and in America psychology was already established as an independent science with laboratory courses. This country was awakening to the importance of this new development. One has to remember that the empirical method was the established mode of British philosophy, and that although psychology had grown up within philosophy it had never been rationalistic or dogmatic in character. The Association School, and particularly the mental chemistry theories of John Stuart Mill, had favoured the collection of data, their analysis and classification. It 'explained' mental phenomena by analysing them as compounded of simple elements. Later, when

¹ Later Sir F. Mott. In this chronicle, in referring to Members I have used the style of address familiar at the time under review. I hope this practice will not seem discourteous. Also I have ignored the military titles of the war years. If I have postdated or antedated knighthoods, professorships and doctorates, may I here and now apologize.

biology took a leading place among the sciences, a different conception of mental phenomena arose. They came to be studied as manifestations of a mental life, and theories of mental development became of paramount importance. By 1901 a biological presentation of psychology had become general. The student following psychology as part of his curriculum for a degree in philosophy was far from ignorant of the early experimental work. He would have found the results cited in the text-books of Sully and Stout or in James Ward's famous article in the *Encyclopaedia Britannica*. Sully's *Human Mind* was a mine of information. It was an acceptable successor to the comprehensive text-books of Alexander Bain, but was written under the influence of biological conceptions. Stout's *Analytic Psychology* was published in 1896. In his search for the essential characteristics of cognitional, affective and conative processes Stout followed the inductive method. It is only after a careful analysis of instances that conclusions are reached concerning the characteristics in question. In 1898 he published the *Manual* which became the standard class-book for generations of students. In a sense it was the complement of the earlier work. It followed the synthetic or genetic method and made use of the relevant data yielded by the laboratory studies of American and German writers and by physiological research. The prominence given to conation and conative unity shows the influence of his teacher, James Ward. Ward's article on psychology appeared in the ninth edition of the *Encyclopaedia Britannica* in 1886. The life of the mind is studied from the standpoint of the living subject in intercourse with his special environment. The activity of the subject, the "I" of "I know", "I feel", "I do" and the principle of subjective selection in relations with the environment is regarded as fundamental for all intellectual growth. Although Ward's psychology is closely linked with his whole philosophic outlook, his presentation of psychology has, nevertheless, a certain completeness and is, up to a point, satisfying. The force of his personality lies behind his exposition and his teaching has had a profound influence on English psychological thought.

In 1901 psychology was receiving a strong stimulus from physiology. Prof. James's books had already shown this clearly. At Cambridge, in 1897, Dr W. H. R. Rivers had been appointed University Lecturer in Experimental Psychology and the Physiology of the Special Senses. Within the physiological laboratory itself he set up what was the first laboratory for psychology in England.¹ His article on "Vision", contributed to Vol. II of Schäfer's *Physiology* (1900), brought much new knowledge within the reach of the student, in particular it gave him a valuable review of the work of Helmholtz and Hering. Psychophysics aimed at determining measurements in the realm of the special senses where the Weber-Fechner Law had shown the importance of thresholds of sensation and of thresholds of difference between one intensity and another. Reaction-time was another direction in which quantitative results were obtainable. Ebbinghaus's experiments on memory and Kraepelin's studies of work-curves were opening up fields in which the experimental method promised to produce new data. There was a reaching after fresh knowledge, a quickening of men's desire to push further. The knowledge to be gained promised to be of practical importance. For the doctor it was important for the study of deviations from the normal acuity of the senses and for the study of abnormalities of behaviour in mental disease. The student of education saw in experiment a new instrument by which to gain

¹ This laboratory was moved to a separate building in St Tibb's Row in 1901. It moved again, in 1903, to a cottage in Mill Lane. In 1913 it once more became associated with physiology. Thanks to the efforts of Dr C. S. Myers, a wing was built for it in the new physiological building. Here it continues to flourish and to expand.

insight into the problems of learning. The anthropologist welcomed a new ally in the study of native people. One of the outstanding cases in which this was demonstrated was the expedition to the Torres Straits organized by Mr Haddon in 1898. Here psychological field work was undertaken by Dr Rivers, and by two men who had worked in the laboratory with him, viz. C. S. Myers and W. McDougall. The association of these three Cambridge men counted for much in the work of the expedition, and subsequently did much to shape the development of the new society.

Such then was the stirring of spirit which led to the desire to form a psychological society whose aim should be to advance scientific psychological research and to further the co-operation of investigators in the different branches of psychology. Membership was to be limited to those who were recognized teachers in some branch of psychology or who had published work of recognized value. The subscription was fixed at half-a-guinea, and a Committee consisting of Prof. Sully, Mrs Bryant, Dr McDougall, Dr Mott and Mr Shand was elected. Mr Shand was appointed Treasurer and Secretary. The Committee was instructed to draw up rules embodying the agreed principles, and at a subsequent meeting these rules were duly passed. The Society was to be governed by a Committee of five, two of whom retired annually though eligible for re-election. The Committee elected its own Chairman and the Treasurer-Secretary was the only officer. An annual general meeting was to be held in January of each year and at least three other meetings during the year at such times as were convenient. Names of candidates for membership were to be brought before the Committee and if approved were voted upon at the next meeting of the Society, one adverse vote in five being exclusive. (This rule still holds.) Honorary members might be elected from time to time. Abstracts of papers to be read were to be distributed to members one week before the meeting.

By the end of its first year, December, 1902, the Society had held six meetings, four at University College, London, one at Cambridge and one at Oxford. It had thirteen members. Following the practice of the Physiological Society, meetings were held in different Colleges on the invitation of the College, and the Chair was taken by the head of the College psychological department. By its third meeting the Society had already fallen into the routine which lasted for many years. The meeting was held in the afternoon (nearly always on Saturday). The two or more papers were 'spaced' by a tea interval, and the meeting was followed by an informal dinner. The arrangements for these dinners were in the hands of Mr Shand, one of the most beloved figures of the early days. Mr Shand possessed all the charm of the Victorian gentleman and was a perfect host. Those who were privileged to attend these early dinners look back upon them as delightful occasions. They were very informal, but the discussions which arose were of great interest and did much to promote mutual understanding and to stimulate new ideas. The women members of the Society were never excluded from these dinner gatherings but for the first few years it seemed to them wiser not to attend. If to-day this point of view is difficult to understand, one must see in it a measure of the change in social relations which time has brought about. One thing is clear to the writer; had the women members attended, those early dinners would have been more formal and the discussions less spontaneous.

In January 1904 the offices of Treasurer and Secretary were separated. Mr W. G. Smith became Treasurer and Dr C. S. Myers Secretary. The first extant minute book of the Committee begins this year in his handwriting. Previous to this date the work of the Committee is represented by a note here and there on the blank side of the minute book

of the Society. At Dr Myers's suggestion a booklet was issued giving the names and addresses of members, the rules of the Society and the forthcoming meetings. It seems probable from the minutes of the Committee that this was first issued in 1904.¹ It was certainly issued in 1906 and was reissued in 1909 and then annually down to 1917. The last issue was in 1919. At the annual general meeting in January 1904 the Society elected its first Honorary Member, Dr J. Hughlings-Jackson, F.R.S. In 1905 Mr Francis Galton, Prof. Höffding, Prof. W. James, Prof. G. E. Müller, Prof. Ribot, Prof. C. Stumpf and Prof. Wundt accepted membership. To these were added Prof. J. Sully in 1910, Prof. Külpe in 1911, and Prof. Brentano and Prof. J. Ward in 1912.

In 1906 the Society changed its name from "The Psychological Society" to "The British Psychological Society". This was not due to any sudden uprising of imperial pride, but to the fact that members had discovered another body of persons who were using the former title. To prevent confusion with this unacademic group the change in title was agreed to.

Having dealt with organization, it is time to consider the psychological work of the Society in this first period of its history. The first paper was read to the Society on Saturday, 15 February 1902, by Prof. James Sully on "The Evolution of Laughter". Looking back one is inclined to think that this inauguration was a happy one. The Society has given rise to much intellectual enjoyment and its discussions have been characterized by good humour wherein laughter has had its role. At the same meeting Dr McDougall gave a paper on "Fechner's Paradoxical Experiment" and Mr W. G. Smith one on "Pathological Changes in Immediate Memory and Association".

In these early years the Society received the support of both philosophers and physiologists. Prof. J. Ward made two contributions. In 1902 he gave a paper on "Psychophysical Parallelism" and in 1904 one on "Subconsciousness"; this latter paper discussed the theory set forth in his *British Encyclopaedia* article. In the summer of 1910 the Society joined with the "Aristotelian Society" and "Mind Association" for a week-end of meetings. There was a very notable symposium on "Instinct and Intelligence" in which Dr C. S. Myers, Prof. Lloyd Morgan, Mr H. W. Carr, Prof. Stout and Dr McDougall took part. The topic was a burning one. Philosophers and psychologists were alike interested. Prof. Bergson's views set forth in the recently published *Evolution Créatrice* had aroused much discussion. Mr H. W. Carr was able to act as his exponent. Each of the other four speakers had given his own psychological interpretation of instinctive action, so that the joint session provided a field day with big guns in action. Another important paper at the same meeting was the one on "The Nature and Development of Attention" by Prof. Dawes Hicks. Mr Bullough made the first of his contributions to the psychology of aesthetics, "The Aesthetic Appreciation of Simple Colour Combinations". The symposium papers were subsequently published in *The British Journal of Psychology*.

A similar joint meeting was held with the two philosophical societies in 1913. A session was devoted to the ever-recurring question "Are Intensity Differences of Sensation Quantitative?" Dr C. S. Myers, Prof. Dawes Hicks, Mr H. J. Watt and Dr W. Brown opened the discussion. Another session was taken up by the question "Can there be Anything Obscure or Implicit in a Mental State?" Prof. Stout upheld the psychologist's view of implicit apprehension.

¹ The earliest copy in my hands gives the programme for 1907. I have not been able to discover any copy of earlier date.

In 1910 Dr Bernard Hart, at a meeting held in Oxford, gave a paper on "The Psychology of Freud and his School". Three years later Mr Pear gave the Society "An Analysis of some Personal Dreams, with Special Reference to Freud's Interpretation" and at the same meeting Dr W. Brown spoke on "The Psychological System of Sigmund Freud, as set forth in Chapter VII of *Traumdeutung*". The new doctrines were arousing increasing attention in medical circles.

From the physiologists also came support for the Society. In 1905 Prof. Gotch read a paper on "The Brain of the Chimpanzee Sally". In 1907 he contributed "A Suggested Physiological Basis for the Distinction between a Sensation and its Revived Image", and in 1910 "A Possible Factor in the Monocular Appreciation of Spatial Depth". In 1903 the Society held a meeting in the pathological laboratory at Claybury Asylum. Here Dr F. W. Mott read a paper on "Bilateral Cortical Lesion, causing Deafness and Aphasia", Dr Robert Armstrong Jones discussed "Various Types of Insanity" and Dr Bolton examined "The Function of the Frontal Lobes". Dr Mott gave the Society further contributions in 1904 and 1905. From Dr Henry Head came two outstanding communications. In 1907 he, with Dr W. H. R. Rivers, gave "Some Points of Psychological Interest Suggested by a Case of Experimental Nerve Division". This to those who heard it was a thrilling account of the now well-known experiment in which Dr Head had been himself the subject. In 1909 he read a paper "On the Grouping of Afferent Impulses in the Spinal Cord".

The Society received early sketches of the theories which have come to be regarded as characteristic of their authors. From Mr Shand it heard papers on "Types of Volition", "A Method of Tracing the Derivation of Secondary Emotions", "A Critical Examination of Ribot's Definition of Passion"—topics which were fundamental for his great book, *The Foundations of Character*. As early as 1905 Prof. Lloyd Morgan was discussing the "Relation of Intelligence to Instinct and to Rational Thought", relations which he brought up again in the symposium in 1910 and set out in full in his book *Instinct and Experience* (1912). At Cambridge, in 1904, Dr McDougall brought before the Society his views on the "Bearing of Modern Experimental Work on the Problem of the Unity of the Mind". In 1906 he discussed "The Fundamental Forms of Mental Interaction". *Body and Mind* was published in 1911. His book on *Social Psychology* appeared in the autumn of 1908, and in 1909 a paper from him on "Some Difficulties Connected with the Current Conceptions of Instinct" gave rise to much discussion. At this period Dr W. H. R. Rivers was concerned with the question of fatigue and with the psychology of the special senses from a comparative point of view, and thus the Society heard from him papers on "The Influence of Alcohol on Muscular and Mental Efficiency", on "Visual Acuity in Different Races of Man", and, with the co-operation of Prof. Dawes Hicks, a paper on "The Illusion of Horizontal and Vertical Lines with Momentary and Prolonged Exposure". Dr C. S. Myers also made contributions on the special senses. In 1904 "Theories of Consonance and Dissonance", in 1905 "The Rhythmical Sense of Primitive People", and in 1909, with Mr C. H. Nicholl, "Observations on Contrast with Smoothly Graded Disks". Prof. Carveth Read gave the Society interesting papers on "Character", 1909, on the "Psychology of Genius", 1911, and on "Wonder, Fascination and Curiosity" in 1913. To this period also belong the first discussions of correlations. In 1909 Dr W. Brown read a paper entitled "An Objective Study of Mathematical Intelligence" and in 1910 a paper on "Statistical Methods". These were basic for his book *Essentials of Mental*

Measurement, published in 1911. In 1910 Dr C. Spearman gave to the Society his first paper. It was on "The Calculation of Correlations", the line of work which has become so closely associated with his name.

In excuse for the above lengthy review of the papers brought before the Society the writer would plead anxiety to recapture for the present and for future members of the Society a sense of the active support given to the young Society by the most outstanding of its members, whether psychologists, physiologists or philosophers.

Papers and discussions at its meetings did not cover all the Society's activities. Its Committee showed concern for psychological questions affecting the general public. In 1906 Dr McDougall brought up for discussion a List of Mental Characters for Anthropometric Investigations in Schools. This list was discussed through two meetings, and it was resolved to appoint members of the Society to co-operate with a Committee set up by the British Association for the purpose of reporting on Anthropological Investigation in the British Isles. This may perhaps be regarded as marking the first interest shown by the Society in what approximated to mental tests.¹ In 1908 the Society set up a Committee to inquire into the adequacy of the present tests for Colour Blindness. In 1913 it called the attention of its members to the inclusion of psychology as a subsection of the Physiological Section of the British Association. From 1906 to 1913 papers on psychological subjects had only been accepted under the guise of physiology.

A question which constantly occupied the attention of the Committee during this pre-war period was that of publication. In 1902 Prof. Stout made a suggestion that the communications read before the Society might be published in *Mind*. A committee was appointed to investigate this possibility. In 1903, however, the Committee learned that there was a scheme for bringing out a psychological journal, and they therefore did not pursue further any negotiations with the "Mind Association". *The British Journal of Psychology* edited by Prof. James Ward and Dr W. H. R. Rivers was first published in 1904. A suggestion that abstracts of the papers read to the Society should be published in this journal was found to be impracticable, since many of the abstracts were too technical and too brief to be intelligible in that form. The *Journal*, however, undertook to publish a list of all communications and to allot a certain space in each number of the *Journal* to the proceedings of the Society. In 1908 the Society approached the Editors with a view to establishing more formal relations between the *Journal* and the Society. Questions of editorial responsibility and questions of finance made negotiations difficult. In 1911 Dr C. S. Myers took the place of Prof. James Ward as co-editor with Dr Rivers. In the same year the *Journal* published its first Monograph Supplement and thus began what has proved a very useful series of research publications. The improving financial position of the Society made it possible to consider whether the Society could not give financial assistance to the *Journal*. In 1913 the Committee discussed the possibility of raising the subscription to the Society from half-a-guinea to a guinea, this higher rate to include the subscription to the *Journal*. A post-card plebiscite of members was favourable to this proposal, and at the annual general meeting of January 1914 this new rate of subscription was adopted and the Society assumed financial responsibility for the *Journal*. Dr C. S. Myers became sole Editor and as Editor was appointed an *ex officio* member of the

¹ It is interesting to note what was happening in Paris. Binet's tests had been published in 1905. January 1906 saw the opening of the laboratory attached to the Normal School in rue Grange aux Belles, Paris. Here Prof. Binet worked further at his scales for the average bodily measurements and mental development of school children and for their scholastic attainments.

Committee of the Society. Thus was accomplished a union which has been an unfailing source of strength to the Society.

Another enterprise undertaken by the Society has also had weighty results. In 1905 the Committee discussed a proposal by the Treasurer to reduce the subscription from 10s. 6d. to 5s. as the expenses of the Society were small. This was in April. Then occurred one of those trivial events from which surprising consequences follow. At a dinner in that month it happened that there were two fewer diners than ordered dinners. The Society decided that these two uneaten dinners must be paid for out of the funds of the Society. This apparently caused the Treasurer to have second thoughts about surplus funds, for in November he told the Committee he did not propose to bring the question of a lower subscription before the Society. In 1906, money still being easy, the Committee made a better suggestion, viz. that money should be used to purchase psychological journals for the use of members. In July of that year £5 was voted for this purpose. Eight journals were ordered. The story of their circulation to members, could it be told in full, would be a moving one.

The names and addresses of members wishing to read any particular journal were printed on a long slip pasted on the front cover. It was the duty of the bookseller to despatch the journal to the member whose name stood first on this list. It was then the duty of this member to forward it within fifteen days to the member whose name stood next. This procedure was to be followed through the list. The last reader had the duty of returning the journal to the Secretary. A perfect plan! Alas, through the frailty of human nature it was very imperfectly executed. The bookseller did his part, but after that anything might happen. There were delays and often full stops. From time to time this vexed question of the circulation of the journals was brought up by aggrieved members. Various resolutions appear in the minutes, all aiming at remedying the evil, and all, one fears, equally unsuccessful. Despite these tribulations some tutelary spirit presided over the fate of the journals. It had originally been intended that they should be sold by auction to members at the end of each year. Wiser counsels prevailed and in 1908 it was decreed that completed volumes should be "bound in stout linen" and lodged under the care of a librarian in some suitable place where they might be accessible to members. Dr W. Brown became the first Librarian and the volumes were deposited at King's College, London. In 1910 the importance of the office was recognized by making the Librarian *ex officio* a member of the Committee. Thus was laid the foundation for what is now one of the Society's most valuable possessions, a reference library of psychological journals.

In view of later developments it is interesting to note a matter which was discussed by the Committee in 1906. The question of establishing a wider society with more popular interests was raised. It was suggested that the existing society should remain as the "research branch" of such an extended society. The discussion did not lead to any practicable scheme and nothing was laid before the annual general meeting in 1907. At that meeting, however, Mr Winch, a member of the Committee, proposed that the rule which limited visitors to one per member should be amended, and that each member should be allowed to introduce three visitors in order to encourage "frequent visits from those interested in psychology, who for one reason or another were inadmissible as members of the Society". This amendment was passed unanimously. It aimed at extending interest in the Society. The extension gained, however, was unaccompanied by increase in income. In January 1914 the membership of the Society stood at 79.

II. CARRYING ON. EXPANDING, 1914-1928

In the spring of 1914 the Society held two joint meetings with the Psychiatric Section of the Royal Society of Medicine, a sign of the growing medical interest in psychology.

In July the Society again joined with the two philosophical societies for a week-end meeting. It was held at Durham and the distance from London demanded the provision of accommodation for members. This was arranged in the University Hall and thus was introduced what has become one of the most enjoyable features of week-end meetings, residence in common in a University Hall with opportunities for social intercourse and informal discussion.

During the war years the Society carried on its regular meetings four times a year. Attendances were not large, but sufficient to make meeting worth while. The Secretary's familiar ending to the minutes of the ordinary meeting undergoes a change. In November 1914 in place of the words "Members subsequently dined together" this strange statement appears: "Members subsequently joined a ladies' dinner at the Grafton Hotel." About 1912 the women members and their friends had adopted the plan of holding a dinner of their own and on this occasion it was *the* dinner. Duplicate dinners were held in January and in March, but in May 1915 the minutes close with the chilly comment "The meeting then ended". Not until March 1918 does one find again the cheery record "Members subsequently dined together at a neighbouring restaurant". One wonders whether the meal at that date can have justified the verb. At least there will have been the sauce of good fellowship.

In January 1915, during Dr Myers's absence on war-service, Mr Cyril Burt took over the editorship of *The British Journal of Psychology*, and Mr Wildon Carr became Treasurer in the absence of Dr McDougall.

In 1918, towards the end of the war, the Society found itself facing serious problems. Much good material awaited publication but, owing to the greatly increased cost of printing, the charge of the *Journal* on the Society was heavy. If the Society was to pay its way it needed more members. As matters stood its membership roll was more likely to decrease than to increase. There was some probability that those members of the medical profession who were interested in functional nervous disorders from their war experiences would found a society of their own for the study of medical psychology and would publish a journal devoted to this subject. In industrial circles also there was a growing desire for the application of psychology to problems of work and of business efficiency. In its present form the Society could not meet the needs either of the medical profession or of the industrialists. The Committee, therefore, asked Dr Myers, who had resumed the editorship of the *Journal*, to draw up a report dealing with the future of the Society, to be laid before the annual general meeting in January 1919. This was done, and a special general meeting was called in February to consider this report together with the recommended changes in the rules of the Society. These changes were twofold. The professional limitation of membership to those *engaged* in the various branches of psychology was to be replaced by a rule opening the Society to all those *interested* in various branches of psychology. A new rule was to be introduced authorizing the formation of sections representative of such lines of interest. In the scheme put forward by the Committee the proposed sections were to be distinct units, membership of any one of which was independent of membership of any other. The original Society was to become a "General Section" collateral with any other—in short the Society would have become

a federation of sections. In the scheme finally adopted the organic unity of the Society was preserved. Membership of the "General Section" was fundamental and membership of any other section was supplementary to this. Three such sections were formed: medical, educational, industrial. The effect of these changes on the membership was all that the Committee could have desired. For whereas at the close of 1918 the number of members was 98, at the close of 1919 it was 427. A very large proportion of these new members were members of the Medical Section, which became nearly as large as that of Education.

The changes in the rules brought with them consequent changes in the Executive Committee. This was now styled the Council and was to consist of a President, Vice-Presidents (past Presidents of less than seven years' standing), Treasurer, General Secretary, Librarian, Editor of *The British Journal of Psychology*, Chairman and Secretary of each Section and six ordinary members.¹ The President was to hold office for three years and to be a Vice-President for the next six years. Two ordinary members were to retire annually but to be eligible for re-election. The subscription remained at one guinea inclusive of the *Journal*. Membership of the special sections required an additional fee, the amount being determined by each section. The sections were allowed associate members who paid the sectional fee only. Such associates did not receive the *Journal* and had no vote in any question concerning the government of the Society. The date of the annual general meeting was changed from January to December. The customary four meetings of the Society meeting as a whole were retained. Candidates for membership required the sponsorship of two members. Such nominations were brought before the Council for approval prior to being put up for election.

In December 1920, under the new rules, Dr Myers was elected as the first President. He retained his position as Editor of the *Journal* until 1924 when Mr F. C. Bartlett succeeded him. Dr T. W. Mitchell became Editor of the new *British Journal of Medical Psychology* which was published in connexion with the Medical Section. In 1922 a fourth section, Aesthetics, was formed on the initiative of Mr E. Bullough. In December 1923 this section had a membership of 58.

In the spring of 1921 the Society allowed itself to break out in a festive manner. It wished to express its appreciation of the services Mr Flugel had rendered to the Society as General Secretary from 1911 to 1920. This period covered the war years and the reconstruction and enlargement of the Society. A dinner was arranged in Mr Flugel's honour and was enjoyed as a friendly, happy gathering. Those who thanked Mr Flugel on this occasion could hardly have foreseen that the Society would owe him a further debt for his future services as Librarian and later as President, but such has been the case.

From 1920 to the end of 1924 the question of premises was continually being brought up in Council. It was felt to be very desirable for the Society to have some headquarters where it could house its library and hold its meetings. The Council owed much to the hospitality of its Treasurer, Dr W. H. B. Stoddart. For five years such of its meetings as could not be pressed into a short hour prior to a general meeting were held at his rooms in Harcourt House, Cavendish Square. Who can say how much was contributed to the deliberations of this period by the handsome parrot who held a watching brief in his corner by the window.

¹ In 1909 the number of ordinary members on the Committee had been raised from the original number of five to seven.

It was learnt in 1924 that the Royal Anthropological Institute were about to purchase the lease of No. 52 Upper Bedford Place, and in December it was agreed that the British Psychological Society should rent from them some of the rooms. In order to be exempt from the payment of rates under the Scientific Societies Act of 1843, it was found to be necessary for the Society to pass a rule prohibiting the payment of any dividend or bonus or gift money to its members. This was passed in 1925. Unfortunately, however, the Royal Anthropological Institute then discovered that by the terms of its own lease it was unable to sublet any part of the house. In view of this difficulty the idea of a headquarters was abandoned. The Society had to content itself with the loan of a small room for housing its library and the use of the Royal Anthropological Institute's lecture room for Council meetings and for occasional meetings. For this accommodation the Society agreed to pay the Royal Anthropological Institute £50 per annum.

A need which had become urgent through the increase in membership and the work of the sections was secretarial assistance. Had the Society secured rooms of its own, it would have appointed a secretary who would also have had charge of the library. Under the terms agreed upon this was impossible. To meet the difficulties the Society made an arrangement with a typewriting bureau for the services of a secretary who should keep the accounts of the Society, attend Council meetings and take the minutes, issue the notices of meetings and give general assistance to the Honorary Secretary. This arrangement was made and continued unchanged until 1928. At this date attendance at Council meetings was dispensed with.

The growth of the Society had not been free from growing pains. Many members felt that although much had been gained in financial stability through the increase in membership, much had also been lost. The informal and intimate form of discussion which had characterized the early years was not possible in the larger gatherings of members who had varying interests and different levels of psychological training. Some of the older members with a background of philosophy felt that some of the papers read lacked breadth of outlook and were trivial in character, even though they purported to have some immediate practical interest. Dissatisfaction with the meetings led in some quarters to a proposal for a new psychological club with a restricted membership. As this seemed likely to be very detrimental to the well-being of the Society, Dr C. S. Myers submitted to the Council a scheme for the institution of a special elected group of members to be styled 'Fellows'. Eight original Fellows, elected on grounds of psychological eminence, were to elect fifteen others. These twenty-three would in each year elect not more than two new Fellows. The number of members in the Fellows group was never to exceed 50. It was hoped that this small group might recapture the atmosphere of the original Society by the informal discussion of technical questions and projected researches before these were presented in a more suitable form to the larger audience of the General Society or one of the special sections. This scheme was brought forward at an extraordinary general meeting in March 1925. As it met with a good deal of criticism, it was decided to postpone its further consideration for one year. It was reconsidered by the Council in 1926, who then decided not to proceed with it.

The Society had not elected any Honorary Members since 1905. In 1926 the names of eight distinguished psychologists were proposed.¹ The President of the Society, Prof. Spearman, suggested that election to Honorary Membership should be marked by the

¹ For full list of Honorary Members see Appendix II (p. 132).

presentation of a special diploma bearing the Society's seal. This was agreed to, and a diploma and suitable seal were designed.

An extension of the Society's influence and an increase in its membership was brought about by the establishment of branches. The first suggestion for a branch came from Scotland in the autumn of 1923. The idea also found favour with Birmingham members, and in February 1924, at an extraordinary general meeting, the Society was given power to sanction the formation of branches. Branches arrange their own meetings and manage their own local affairs, but are required to submit their rules to the Council and to present an annual report. Members of branches pay the ordinary subscription to the Treasurer of the Society, the Council granting permission to incur such expenses as may be necessary for the work of the branch. In December 1927 branches were allowed to elect associate members on the same terms as the sections. Despite the great improvement in finances, there was a moment in 1920 when the Treasurer seriously suggested to the Council that the Society should no longer continue to provide tea free of charge at the general meetings of the Society. The Council decided to "continue the practice for the present". The subject does not reappear in the minutes and a major calamity to the social life of the Society was averted.

The first report by the Treasurer to be incorporated in the minutes of the annual general meeting is for 1922. It is very brief, but showed a satisfactory balance on the current account and £550 on deposit. It was not until 1925 that an analysis of the accounts was presented to the Society. The increase in the Society's resources and in its expenditure made it desirable to seek expert advice on the way in which its accounts should be kept. Up to December 1926 the audit each year had been undertaken by some reliable and painstaking member of the Society. From 1926 onwards that duty has been performed by a chartered accountant. In 1925 a life composition fee was instituted. One enterprising member availed himself of it, but no other member was tempted to follow his example. The scheme was abolished in 1932 and the financial situation adjusted with the one and only life member.

Owing to the absence on war-service of members of the psychological staff of King's College, the library was moved to University College in 1917 and remained there until 1926. The hazardous practice of circulating periodicals was continued until 1916 when it had to be given up, owing to the difficulties caused by the absence of members abroad and the increased cost of postage. In 1922 Dr Flugel was appointed Librarian and began the arduous task of overhauling the Society's collection of journals. Arrears of binding had to be made up, missing numbers sought for and purchased—a work of time and patience. In 1926 he was able to report that the library was in order and was installed in the small room at 52 Upper Bedford Place.

Rules for the use of the library were the next requirement. A committee drafted three alternative schemes. By Scheme A the borrowing of volumes was prohibited. By Scheme B borrowing for fourteen days by a personal application was allowed. By Scheme C borrowing by post was permissible for members living more than twenty miles from Charing Cross. The Council adopted Scheme A, and in view of the difficulties already experienced this seems to have been a wise decision. In 1927 the Librarian made the first of those reports which are now presented to the Society every year. He was able to report that the Society possessed 684 bound volumes and 106 in cardboard storage cases. These represented 93 separate publications, 16 sets of which were seriously incomplete.

The work of the Society for the advance of psychological knowledge was greatly aided by the improvement in its library, due to the efforts of the Librarian.

Through the support given to the proposal by the British Psychological Society, Psychology was established as an independent section of the British Association in 1921.

The Society became in 1917 one of the constituent societies of the Board of Scientific Societies and was represented on that body by Dr W. McDougall. In 1920, through Dr Myers, it was also represented on the board of management of a new journal, *Discovery*, which dealt with the recent advances in scientific knowledge.

Dr Myers was the Society's representative on the committee formed to consider a memorial to the late Dr W. H. R. Rivers. A fund was raised for the furtherance of the sciences to which Dr Rivers had been interested, in particular anthropology and psychology. The President of the British Psychological Society is an *ex officio* member of the committee administering this fund.

University psychological societies were springing up as psychology became more widely studied. That of the University of London was affiliated to the Society in 1923 and its members, who were required to be registered students reading for a degree, were allowed to attend all meetings of the General Society and of such sections as would accept them.

Turning now to the more strictly psychological work of the Society itself during this period, one must notice in the first place the formation in 1923 of a standing committee called the Committee for Research in Education. This was formed at the instance of the Education Section and was sponsored by Mrs Susan Isaacs. It was due to her energy and ability that it did useful service for education. Two-thirds of its members were required to be members of the Society; the remaining third were co-opted and were chosen for their ability to help the committee in its work. This body received an annual grant from the funds of the Society, and from 1929 onwards had a representative on the Council. It served as a bureau of inquiry for educationalists, and every year it published a list of the psychological research work then in progress on educational problems. This committee continued in being until 1935 when it was judged that its work could be more appropriately carried on by other bodies.

The Society began a new venture for the advancement of psychological research when in 1925 it made its first grant to assist the publication of one of the *British Journal of Psychology Monograph Supplements*.

Many papers of the period under review were concerned with some aspect of Freudian or of analytic psychology. Thus, in March 1914, Dr McDougall discussed "The Definition of the Sexual Instinct" at the meeting held with the Psychiatry Section of the Royal Society of Medicine. At the Durham joint meeting with the two philosophical societies a session was devoted to "The Role of Repression in Forgetting". At the London meeting in 1918 a symposium was held on "Why is the Unconscious Unconscious?" Dr E. Jones, Dr W. H. R. Rivers and Dr Maurice Nicholl gave their views of the answer. In 1919, at the joint meeting, held again in London, the cognate theme of "Instinct and the Unconscious" was discussed. The contributors were Dr W. H. R. Rivers, Dr C. S. Myers, Prof. Jung, Mr Graham Wallas and Dr J. Drever.

In 1920 the Society took part, together with the two philosophical societies, in a Congress of Philosophy held at Oxford. The University philosophical club, the Société Française de Philosophie and the Société Française de Psychologie were also members of the Congress.

There were three important psychological sessions: A paper by Dr Henry Head and Dr W. H. R. Rivers entitled "Disorders in Symbolic Thinking due to Local Lesions in the Brain" gave the Society the results of research work on war patients. The newly formulated doctrines of Behaviourism were discussed in a symposium "Is Thinking merely the Action of Language Mechanisms?" Mr F. C. Bartlett, Mrs Bartlett, Prof. Godfrey Thomson, Prof. Robinson and Prof. Watson contributed papers. Unfortunately the last-named was unable to defend his thesis in person. The aesthetic interests of psychology were represented by a symposium on "Mind and Medium in Art".

The 'new' psychology again found a place in the joint meeting held in Manchester University in 1922. The question "Is the Conception of the Unconscious of Value in Psychology?" and the problem of "The Relation between Sentiments and Complexes" were debated by two brilliant teams: Messrs Field, Aveling and Laird, and Messrs Rivers, Tansley, Pear, Hart, Shand, Myers. At this meeting a day was devoted to experimental work. Subjects as diverse as "Musical Appreciation" and "The Effect on Mental Fatigue of Rest Pauses of Varying Duration and Quality" were handled. Of more philosophic interest was Prof. Stout's acute criticism of "Mr Alexander's Theory of Sense-Perception". The presence of "Mr Alexander" supplied an added piquancy to the discussion. This was the last of the joint meetings held with the two philosophical societies, the Aristotelean Society and Mind Association. It was in some respects one of the most outstanding, but the tendency for attendance at the different sessions to be rather one-sided showed that the divergence of interest was becoming too wide for common week-end meetings to be profitable to both philosophers and psychologists.

Other contributions which dealt with the theories of Freud and Jung were a paper on "Symbolism" by Dr E. Jones in 1915, and one on "Destructiveness and Superstition" by Dr Eder in 1918. At the same time Dr W. H. R. Rivers discussed "Dreams and Primitive Culture", and in 1919 "The Freudian Conception of the Censor". In 1921 he took up the subject of "Dream Analysis". In 1924 Dr Seligman brought to the notice of the Society "The Psychology of the Unconscious in relation to Anthropology".

The contemporary interest in psycho-galvanic reactions was represented by two papers from Mr Thouless in 1924 and 1925, one from Miss H. Wells in 1924 and one from Mr R. J. Bartlett in 1926. A demonstration by Dr Prideaux in 1921 and a paper by Miss Stoneman in 1925 dealt with psycho-galvanic responses of asylum patients. The influence of the new Aesthetic Section was seen in papers given to the Society by eminent writers. In 1923 Sir Roger Fry told the Society "What Artists want from Psycho-analysis". In the same year Sir Charles Walston expounded "Some Aspects of the Philosophy of Harmonism". In 1925 Mrs Ursula Roberts discussed "Intuition and Beauty" and in 1926 Prof. Alexander dealt with "The Creative Process in the Artist's Mind". The new doctrines of Gestalt psychology were brought before the Society by Prof. Spearman on two occasions, in October 1924 and in June 1926.

Papers of the experimental type were less conspicuous than in the previous period. During the first enthusiasm for psycho-analysis with its startling revelations, the results obtained by the often laborious method of experiment seemed dull and even insignificant. Nevertheless, good experimental work was done.

Some at least should be mentioned. "An Experimental Study of some Problems of Perceiving and Imaging" by Mr F. C. Bartlett, 1916. The data then brought before the Society later found a place in his book *Remembering*. A paper on "Practice, Fatigue,

Oscillation" by Mr Flugel was expanded into a monograph publication. So also were the papers on "Fluctuations in Work Curves" read to the Society by Mr Philpott in 1921, 1922 and 1926. In 1921 Mr Boyd-Barrett gave an account of his experiments showing "The Reaction of the Will to Objective and Subjective Stimuli". The outcome of his research was embodied in his book *Motive Force and Motivation Tracts*. A cognate subject was dealt with in 1925 by Miss H. Wells "The Influence of the Relation between Values and Choices". This research also was published as one of the *Monograph Supplements of The British Journal of Psychology*.

Towards the end of the period under review there was evidence of the growing interest in the subject of mental tests. Miss McFarlane gave an account of "Tests of Practical Ability" in 1921 and in 1923 a paper on "The Use of Mental Tests in American Schools and Clinics". In 1926 Miss Collie discussed "The Upper Limit of the Growth of Intelligence".

Like the Society as a whole, the Education Section was much interested in problems of psycho-analysis. It held four meetings jointly with the Medical Section.¹ One of these was outstanding in view of the question raised by Dr Cyril Burt's book *The Young Delinquent* published in 1925. It was a discussion meeting in 1926 on "The Definition and Diagnosis of Moral Imbecility". Dr Burt was the opener of the debate. The Section received two visits from Madame Montessori. She spoke on "Child Psychology" and on "Intelligence Tests". Dr Ballard gave the section several important papers dealing with this latter topic. The Industrial Section concerned itself strictly with practical problems, fatigue, rest pauses, motor capacity, vocational tests. A new note was struck in 1921 by Dr Millais Culpin who discussed "Occupational Neuroses".

Between the years 1924 and 1928 the Society had the privilege of receiving communications from distinguished visitors. At a meeting in Cambridge in 1924 Prof. Koffka gave a paper on "Introspection and the Methods of Psychology", and at the same meeting Prof. Morton Prince spoke on "Meaning". In 1927, in London, Prof. Piaget communicated his study of "La Première Année de l'Enfant". In the same year Prof. Michotte marked his election as an Honorary Member by a paper entitled "Experiments in Learning and Performing Skilled Movements". At a joint meeting with the Industrial Section in 1927 Dr Bingham gave a paper on "Some Individual Differences in Susceptibility to Accidents". At the annual general meeting of the same year Prof. Mary Winton Calkins stated the case for "Self-Psychology". In 1928 Dr Decroly read a paper to a joint meeting with the Educational Section on "La Globalisation dans l'Écriture et la Lecture".

The presence of such visitors is evidence of the extension of the Society's sphere of influence and of the growing recognition of its importance.

III. CONSOLIDATION. SELF-CONSCIOUSNESS, 1928-1941

These thirteen years have seen further extension of the Society's activities. The first important evidence of this was the publication of a third journal, viz. *The British Journal of Educational Psychology*. The Educational Section had pressed for the publication of a larger number of educational articles in the *Journal*. They wished to see a set of pages in each number of the *Journal* assigned to educational topics. The Editor was, however, already experiencing a difficulty in finding space for the number of articles which he

¹ For an account of the Medical Section of the British Psychological Society readers should consult the article bearing that title written by J. Rickman, M.A., M.D., in the series *British Medical Societies, Medical Press and Circular* (September 1938).

wished to accept. After somewhat protracted negotiations an agreement was reached in 1931 with the Training College Association for the publication of a new journal. It was to receive the support of that Association and of the Education Section of the Society. A private limited company, representative of both bodies, became the proprietors of the new journal which took the place of *The Forum of Education*, the journal previously supported by the Training College Association. The editor of *The Forum*, Prof. Valentine, became the editor of the new journal, and the subscription rate of the Education Section was raised in order that it might include the section journal, in the same way as the subscription of the Medical Section included *The British Journal of Medical Psychology*.

In 1931 Prof. Drever was appointed Editor for the *Monograph Supplements* which had been issued from time to time. Applications for grants in aid of publication had become more numerous, and it became necessary to relieve the Editor of the General Section of the *Journal* by the appointment of a separate editor who should adjudicate on the MSS. submitted and be responsible for all publications in the Monograph series. The usefulness of the *Journal* was increased by the publication of an index to the first twenty volumes. This compilation was admirably carried out in 1930 by Dr M. Drury Smith.

The British section of *The International Psychological Register* published in 1929, the year of the International Psychological Congress held at Yale University, was considered very unsatisfactory by the Council of the Society. At the same time the Council realized that it was not in a position to assume responsibility for the list of names of British psychologists to be included in *The International Register* as it had no official information about the status of its members. The matter gave rise to much discussion. The old suggestion of forming a group of Fellows was once more considered. This was not adopted, but it was agreed that the Council should recommend the compilation of a register of all its members and that such a register should show members' degrees, diplomas, academic standing, publications and membership of other learned societies. The Society endorsed this recommendation at an extraordinary general meeting in April 1930. Forms on which to record the desired information were issued to all members, and were completed by July 1931. The information asked for was supplied by 82% of the existing members and carefully filed. From 1931 onwards all new members, prior to election, were required to fill in similar forms. The Society is thus in a position to know the status of individual members and also the line of work in which they may have specialized. This information has enabled the Society to put any research worker who wishes to secure the co-operation of others in touch with those members who are in a position to give the assistance or advice desired.

The large room at 52 Upper Bedford Place, though suitable for Council meetings, did not commend itself to members as a room for general or sectional meetings. The Council, therefore, in 1930 reverted to the former practice of holding meetings in the London Colleges. The small upstairs room allotted to the Society for its library found small favour as a reading room. It became questionable whether the use made of the facilities given to the Society at 52 Upper Bedford Place justified the sum paid to the Royal Anthropological Institute as rent. The growing secretarial work needed more supervision and demanded more time than it was reasonable to expect from the Honorary Secretary. Office expenses with the Typewriting Bureau were heavy, and it seemed as if the Society would benefit in many ways by the appointment of a permanent paid secretary. It was, therefore, decided to seek premises which would make this possible. An office and two

intercommunicating rooms, suitable for use as the Library and meeting room, were rented at 55 Russell Square. The Aristotelean Society, who were also seeking for a new place of meeting, agreed to make use of the larger room for their meetings, and to pay the Society for such accommodation. The rooms at 55 Russell Square served as the Society's Headquarters from 1931 to June 1941. Miss B. Addinsell was appointed Secretary and took charge of all the office work. The library was more and more used by readers, and the increasing number of calls on the Secretary both in person and by telephone showed that the Society's Headquarters were being regarded as a bureau of information on psychological questions by outsiders as well as by members.

In 1932 two new types of meeting were introduced, evening lectures on topics of general psychological interest given by some recognized authority on the topic chosen, and evening discussion meetings whereat one paper only was read, an abstract of which was available in advance. In connexion with these meetings, an attempt was made to revive the old practice of members dining together. It was arranged that a dinner should be provided at a neighbouring restaurant before the meeting. Perhaps a dinner *before* the meeting lacked something of the magic of a dinner after a lively discussion. Perhaps the dinner fell short of the desired standard. Perhaps what was lacking was a Mr Shand. Whatever the reason, the scheme did not meet with sufficient support to justify its continuance. The one section of the Society which up to 1939 was successful in carrying on the old practice of friendly dinners was the Industrial Section. Possibly a section supplies an essential condition by appealing to a smaller number of persons possessing a common interest.

In 1931 the Society extended its field of meetings by visiting the University of Reading, and in 1933 it visited Bethlem Royal Hospital at Eden Park, Beckenham. This was the first visit of the Society as a whole to a mental hospital since the visit to Claybury in 1903.

Another new venture was the arrangement of week-end "extended general meetings". The first took place at Leeds in 1936, lasting from Friday, 17 April to Monday, 20 April. Seven sessions were arranged and the meeting was highly successful. Members lodged at the University Hall and enjoyed the community life thus provided. This week-end meeting was in a sense the revival of the former 'joint' summer meetings. It proved so acceptable that an extended meeting has been arranged each year in the Easter vacation. In 1937 it was held at Manchester, in 1938 at St Andrew's, in 1939 at Reading. On this occasion the meeting was further extended, lasting from Thursday until Tuesday morning. This meeting set a final stamp of success on the new venture. The ambitious plan for 1940 was a long week-end visit to Louvain on the invitation of Prof. Michotte. Like other desirable plans it had to be abandoned owing to the war. In its place a very successful meeting was held at Birmingham from 4 April to 8 April. In 1941 the Extended General Meeting was held at Nottingham from 17 April to 21 April. Coming after a winter in which war conditions had made evening meetings almost an impossibility in big cities, this opportunity for meeting and discussion was greatly appreciated. The large attendance, the liveliness of the discussions and the spirit of good fellowship which characterizes these gatherings make them an outstanding feature in the year's programme. They entail hard work for the host and for the officers of the Society, but the work has been given ungrudgingly and has proved well worth while.

In 1931, a standing order of Council was passed to put the election of Honorary Members on a more formal basis. It was agreed to revise the list every three years. The new candidates were to be put forward by a committee consisting of the President, Vice-Presidents,

and such past Presidents as might be members of the Council and the Chairmen of the several Sections. In 1934 this Committee pointed out that in recent years there had been a tendency to recommend only foreign psychologists. They put forward the names of eight distinguished British psychologists. In this list three names stand out, not only as distinguished psychologists but also as men to whom the Society is greatly indebted: Mr A. F. Shand, Prof. W. McDougall and Dr C. S. Myers. The first two contributed much to the success of the Society in the years before 1914 both by personal service and by the papers they read. Dr C. S. Myers worked for the Society throughout its existence. No other member served so constantly on its Committee and Council. For eight years he was Secretary, he edited the Society's *Journal* for thirteen years and was the pioneer President elected in 1920 after the enlargement of the Society.¹

In 1935 the Society approved the institution of a new office, viz. that of Deputy President. The new office gave the Society a wider range of choice in its selection of president. With the provision of a deputy it was no longer so essential to choose a president who was within easy reach of London. In one direction the Society curtailed its activities. In November 1937 it was decided to give up the Aesthetic Section owing to lack of general support.

As in the previous periods under review, the papers read before the Society and its sections covered a wide range of topics. One can, nevertheless, generalize and say that the period is characterized by the study of quantitative methods. A great impetus was given to such work by Prof. Spearman's 'two-factor' theory as developed in his books *The Nature of Intelligence and the Principles of Cognition* (1923) and *The Abilities of Man* (1927). The two factors 'g' and 's' have now gathered round them an array of other significant letters. Intelligence tests, emotional and temperamental assessments have furnished material for factorial analysis, whether this be undertaken as a search for simple traits or as a method of descriptive classification.

This direction of the interest of the Society was reflected in a new standing committee which was established in 1934, "The Committee on Human Mental Measurements". This committee was instituted for the study of a particular aspect of psychology, i.e. individual differences of a measurable kind, and worked in connexion with the International Federation of Eugenic Organizations. It issued bulletins from time to time, giving full information of the various studies in progress in the British psychological laboratories, thereby continuing on a wider scale one of the functions of the old Committee for Research in Education.

Several of the papers read before the Society were concerned with examinations, their suitability for particular purposes and the reliability of the results.

Child Psychology figured prominently not only in the Education Section but also in the meetings of the whole Society. Mrs Susan Isaacs brought before members "Recent Advances in Child Psychology", Dr Charlotte Bühler lectured on "Development and Personality in Childhood", and Miss Grace Calver dealt with "The Diagnostic and Therapeutic Value of Play". Dr Lowenfeld discussed "The World Pictures of Children" at a meeting of the Medical Section.

At the Manchester Extended General Meeting a session was devoted to "Gestalt Psychology", the opening papers being given by Mr Wolters and Prof. Spearman. The subject was also taken up in different connexions at other meetings. Perhaps the

¹ For list of Honorary Members, see Appendix II (p. 132).

most noteworthy was "Gestalt Theory and Aesthetics", a paper given in 1937 by Mrs H. Knight.

Mental Testing was a constant theme of discussion, particularly in the Education Section. The spread of public interest in tests, whether those used in a clinic or those used for vocational guidance or selection, brought with it a certain danger. Many psychologists realized that some of those who were actually using tests for various purposes, and even devising new tests, had not had the psychological training necessary to safeguard the methods from abuse and the public from exploitation. In 1934 the "Professional Status Committee" was set up to inquire into this problem. In 1937 during the Extended General Meeting at Manchester a morning session was occupied with a discussion of the question, "What is a Qualified Psychologist and for What Work should he be Qualified?" There was a general feeling that the Society ought to protect the interests of trained psychologists, and that by requiring a high standard of professional training from everyone admitted to a certain category of membership, it would enable the public who made use of the services of such members to be ensured against inefficiency and charlatanism.

The existing register might be regarded as providing a first step towards differentiation in the qualification of members, but the Council realized that, if the Society was to accept the function of safeguarding professional interests, it would require to alter its constitution. From 1936 to the end of the period under review the "Professional Status Committee" and the Council spent much time considering this question of constitution. Their labours resulted in a draft constitution which enabled the Society to seek incorporation. The proposed constitution, while it safeguarded the rights and privileges of all existing members, enabled the Council to institute different classes of membership and thereby to establish what may be briefly described as "a professional register".

The draft of this constitution was laid before members at the Extended General Meeting held at Nottingham, April 1941, and met with their approval. On 31 May 1941, at a meeting in London, the Society formally passed a resolution authorizing the Society to transfer the property of the old Society to a Society incorporated under the Companies Act as a company limited by guarantee and not having a share capital, with a Memorandum and Articles of Association.

At the outbreak of the 1939 war the Secretary's office in Russell Square had to be given up for A.R.P. use, but the business and correspondence were for a time carried on by the Secretary from her own home. The Library was sent, in November 1939, to University College, Nottingham, where through the generosity of the College authorities and of the librarian, Mr Flack, it has been cared for pending new accommodation. In 1938-39 the Society had had before it the prospect of more adequate headquarters in the new buildings being erected for the British Medical Society. In normal conditions these would have been ready in September 1940, but building had to be discontinued. As an emergency measure three small rooms in the older building were rented in June 1941.

In November 1938 the Society lost one of its Honorary Members and one who had belonged to it from its formation. This is not the place to write an appreciation of Prof. William McDougall as a psychologist. Here it must suffice to chronicle his name as one who did yeoman service in the early years of the Society's life. In October 1940 the Society lost another of its early members, Sir Henry Head. Although he had not been able to take any active share in the work of the Society for some years, Sir Henry Head

retained his interest in its development and accepted Honorary Membership in 1937. In March 1941 the Society had to record the death of one to whose work it owed much, Prof. Aveling. He had acted as its Secretary from 1920 to 1926 and as its President from 1926 to 1929. His presence at meetings was always welcome both because of his shrewd contributions to the subjects under discussion and, perhaps even more, because of his personality, which had much of the charm and freshness of a boy.

Although after September 1939 the grip of a European war partially paralysed the activities of the Society, the extended general meetings already referred to show that, as in the last war, the Society 'carried on'. At the Extended General Meeting at Birmingham, 1940, a new section was formed, viz. Social Psychology. The purpose of this section is to promote research in social psychology and to foster discussion about the application of psychology to the problems of society.

The Industrial Section meeting in London has kept up its discussions and its social habits by holding short luncheon-hour meetings with coffee and sandwiches available for those notifying their intention of attending.

To the great regret of many the International Congress planned to take place in Edinburgh under the presidency of Prof. Drever has had to be abandoned for the present. One can wish for nothing better than the realization of the plan after the war, and, further, that it may be carried out in the same spirit as that which animated the Congress held in Oxford in 1923 under the presidency of Prof. Myers. There the psychologists of several nations, including the belligerents, met as psychologists resolved to overcome the barriers of war by their common interest and determined good will. Psychology has its part to play both now and later. One may hope that the newly incorporated Society as a whole, and the Social Section in particular, may be able to promote the clearer thinking and better understanding of social and international relations on which the future peace of the world depends.

On 1 October 1941 the official document of incorporation was received from the Registrar of Companies, certifying that the British Psychological Society is this day incorporated under the Companies Act of 1929 and that the Company is Limited.

The King is dead, long live the King!

(*Manuscript received 1941*)

APPENDIX I

List of Officers: Presidents, Deputy Presidents, Editors, Secretaries, Treasurers and Librarians, in Chronological order.

Presidents

C. S. Myers	Dec. 1920—Dec. 1923.	A. W. Wolters	Dec. 1938—Dec. 1941.
C. Spearman	Dec. 1923—Dec. 1926.	C. Burt	Dec. 1941—Dec. 1943.
F. Aveling	Dec. 1926—Dec. 1929.	M. Culpin	Dec. 1943—Dec. 1944.
Beatrice Edgell	Dec. 1929—Dec. 1932.	G. H. Thomson	Dec. 1944—Dec. 1945.
J. C. Flugel	Dec. 1932—Dec. 1935.	R. J. Bartlett	Dec. 1945 to date
J. Drever	Dec. 1935—Dec. 1938.		

Deputy Presidents

A. W. Wolters	Dec. 1936—Dec. 1939.	R. J. Bartlett	Dec. 1943—Dec. 1945.
M. Culpin	Dec. 1939—Dec. 1943.	S. J. F. Philpott	Dec. 1945 to date

*Editors**The British Journal of Psychology:*

James Ward and W. H. B. Rivers	1904—1910.	C. S. Myers	1913.
W. H. R. Rivers and C. S. Myers	1911—1912.		

The British Journal of Psychology (Psychological Society financially responsible for the *Journal*):

C. S. Myers	1914—1924.	F. C. Bartlett	1924 to date.
C. Burt (Acting Editor)	1915—1918.		

The British Journal of Medical Psychology:

T. W. Mitchell	1920—1935.	John Rickman	1936 to date.
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The British Journal of Educational Psychology:

C. W. Valentine	1931 to date.
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Secretaries

A. F. Shand	Oct. 1901—Jan. 1904.	F. Aveling	Dec. 1920—Dec. 1926.
C. S. Myers	Jan. 1904—Jan. 1911.	R. J. Bartlett	Dec. 1926—Dec. 1943.
J. C. Flugel	Jan. 1911—Dec. 1920.	D. W. Harding	Dec. 1943 to date.

Treasurers

A. F. Shand	Oct. 1901—Jan. 1903.	H. Wildon Carr	Jan. 1915—Jan. 1918.
W. G. Smith	Jan. 1903—Jan. 1905.	W. H. B. Stoddart	Jan. 1918—Dec. 1928.
W. McDougall	Jan. 1905—Jan. 1908.	S. J. F. Philpott	Dec. 1928—Dec. 1945.
W. H. Winch	Jan. 1908—Jan. 1911.	A. Rodger	Dec. 1945 to date.
W. McDougall	Jan. 1911—Jan. 1915.		

Librarians

C. Sully	Jan. 1912—Jan. 1916.	E. Jones	Jan. 1919—Dec. 1922.
M. Jane Reaney	Jan. 1916—Jan. 1917.	J. C. Flugel	Dec. 1922—Dec. 1932.
A. Wohlgemuth	Jan. 1917—Jan. 1919.	May Smith	Dec. 1932 to date.

APPENDIX II

List of Honorary Members with dates of election

1904	Dr J. Hughlings Jackson.	1932	Prof. Angell.
1905	Mr Francis Galton.		Prof. J. McKeen Cattell.
	Prof. H. Höfding.		Prof. Sante de Santis.
	Prof. W. James.		Prof. W. Stern.
	Prof. G. E. Müller.	1934	Dr H. Havelock Ellis.
	Prof. Th. Ribot.		Dr Ernest Jones.
	Prof. C. Stumpf.		Prof. F. Krueger.
	Prof. W. Wundt.		Prof. W. McDougall.
1910	Prof. J. Sully.		Prof. C. Lloyd Morgan.
1911	Prof. O. Külpe.		Prof. C. S. Myers.
1912	Prof. Brentano.		Mr A. F. Shand.
	Prof. James Ward.		Prof. C. Spearman.
1926	Prof. E. Claparède.		Prof. G. F. Stout.
	Prof. S. Freud.	1937	Prof. S. Alexander.
	Prof. G. Heymans.		Sir H. Head.
	Prof. le Docteur Pierre Janet.		Sir C. S. Sherrington.
	Prof. Dr H. Piéron.	1940	Prof. Dr G. Dumas.
	Prof. E. B. Titchener.		Prof. Beatrice Edgell.
	Prof. E. L. Thorndike.		Prof. C. E. Seashore.
	Prof. Dr H. Zwaardemaker.	1946	Prof. C. G. Jung.
1927	Prof. A. Michotte.		Sir William Mitchell.
1928	Prof. Mary Calkins.		

THE RELATIONSHIP BETWEEN PERSONALITY AND 'PSYCHOLOGICAL ABILITY'

By J. WEDECK

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I. INTRODUCTION

(1) *The Problem*

The main problem of the first part of this research, which was begun some sixteen years ago under Spearman at University College, London, was whether 'psychological ability' involved simply 'general intelligence' or also a special ability or factor.

(2) '*Psychological ability*'

We understand by 'psychological ability' an ability to judge correctly the feelings, moods, motivations of individuals. But since we cannot enter directly into the psyche of any person other than ourselves, the question arises: How do we come to know the 'meaning' of another's behaviour at all? Koffka's⁽¹⁾ theory that an infant's innate configurations are made to function by an act of the same kind on the part of another individual may be sufficient to account for its response, though McDougall⁽²⁾ would prefer an explanation in terms of 'primitive sympathy'.

But when we consider not merely *response*, but *judgement* of individuals, and more especially judgement in those complex situations into which enter *persona* and feigning, a further process of interpretation would appear to be involved. Before we can give the true interpretation of *any* class of indication, feelings, moods, motives, it would be necessary to know sufficiently the personality of the subject, as Bain⁽³⁾ long ago pointed out. We must take into account also character factors, such as temperament and intellect, which have an effect on the manner and amount of 'expression'; the same feeling may not prompt a similar or equal display in all individuals.

It would seem, then, that in order to judge expression it is necessary to understand personality; and this understanding, according to McDougall⁽²⁾, Spearman⁽⁴⁾, Bain⁽³⁾ and others, is reached in terms of the judge's experiences of himself. To assess correctly the personalities of a wide variety of people, the judge must have attained a high degree of complexity, as well as insight into his own motives; he would need to have a high 'psychological ability'.

To measure this the present author's tests were devised.

II. THE TESTS USED¹(1) *Tests of 'psychological ability'*

Since the subjects most easily obtainable at the time were children of secondary schools², the material for the tests was chosen primarily to suit these (though in the later research it was found equally suitable for adults). The choice of subjects, children about 13 or 14 years of age, was influenced, also, by the problem of reference values; 'g' tests aimed at measuring the cognitive powers of individuals up to the end of school-age had been perfected to a degree which made such tests far more reliable than those which aimed at testing the more mature levels of cognition.

For my subjects I devised a series of five pictorial tests, four of which employed works by well-known contemporary artists, as e.g. Lawson Wood, Mabel Lucy Attwell, Beatrice Mallett.³ In the fifth test I used photographs ('stills') of popular film actors represented in psychological situations.⁴

A criterion employed in the selection of a picture for a test item was the complete agreement between the interpretation of three trained psychologists (including the present writer) and the artist regarding that picture.

Test 1 (H). In this, the first of my pictorial tests, each subject was given a booklet of pictures and a corresponding questionnaire. Each booklet contained forty little pictures covering a wide range of facial expression, starting from such as could be easily recognized, such as laughter, to much more subtle expressions of doubt, curiosity, vexation, etc. Each picture with its corresponding question constituted a test item. Out of the set of four words, phrases, or sentences supplied with the picture the subjects were required to select and underline that one which they considered to suit the picture best. Test item 3, for example, represents a child crying. The words given with the picture are: (a) slightly uncomfortable, (b) very unhappy, (c) gloomy, (d) sulky.

Each test item was so devised that one out of the four alternatives given with the picture was the most suitable for the pictorial representation, in this case (b). In a later item (No. 11), a coquettish young woman was represented in the act of using her powder-puff in a manner that seemed to imply a consciousness on her part of power to excite admiration. The total psychological situation might be expressed verbally: "I think I'm looking rather nice" or "I think I'll look rather nice" (future). The young lady in the picture seemed to be quite at ease, taking the observer into her confidence; this is best expressed in words by the use of the colloquial form of the verb ("I'll"). Moreover, her action seemed to suggest a preparation for some event of importance to her, the meeting with someone whose admiration she desired. Hence the future tense appeared preferable to the present in relation to the situation represented, though, admittedly, the choice between the present and the future tense is a fine one even for the trained psychologist. In the test, however, the subjects were not required to distinguish such a fine point in this item, but only to select the most suitable interpretation among the four given, three of which were inserted as 'catches' or 'traps'. All the items in this test were required to be dealt with by the subjects in the same way as the two examples given above.

A test item was devised in the following way. The suggestion of the total form in the picture was the one upon which the 'correct' verbal expression was based, i.e. the interpretation agreed upon by the three previously mentioned psychologists in regard to the artist's ascription of, and success in conveying, motives to his pictorial characters. The other three verbal expressions that completed the test item were based on suggestions made by the experimenter, based upon a greater or lesser portion of the total pictorial form. By presenting, in addition to the correct verbal interpretation, three incorrect ones remote in meaning from that of the total picture, an 'easy' item was prepared. When they were more nearly related to the

¹ A complete copy of all the tests and test material is lodged in the University of London Library together with a thesis by the present writer.

² This term includes 'Central' schools. The subjects of these tests described here were mentally above the level of 'Senior' school children, whose attainment in reading, too often low, might have prejudiced the results of the testing.

³ By courtesy of Raphael Tuck, Valentine, Faulkner.

⁴ By courtesy of Gaumont, Ltd.

correct solution, but differed only in one or two details from the latter, a more difficult item was devised. The easiest items were kept at or near the beginning of the test, the more difficult ones being put later. Thus the test was graded for difficulty.

Test 2 (I). The test material for this was similar to that of test 1 (H) though on the whole more difficult. Booklet I, used in this test, contained 40 pictures, but the test consisted of 47 items, since in seven cases two questions were asked with each of the seven psychological situations portrayed.

The preparation of the test material and key, and the conduct of the test were similar respectively to those operations in the previous test.

Test 3 (J). This was similar in all respects to tests H and I, with the exception that instead of referring to temporary feelings or moods represented, the questions on each item were concerned with habitual or constitutional tendencies, e.g. hard-heartedness, asceticism, self-consciousness.

With each test item, two, or at most, three of the eight tendencies described in the questionnaire had to be marked as most consonant with the character of the person represented. In all, seventy-five such little answers were required.

Test 4 (K). The material for this test and the key for scoring were prepared as in the case of the three previous tests.

The conduct of this test differed to some extent from that of test J. In test K each subject was given a printed sheet (referred to below as the Explanatory Sheet) bearing a list of twelve persons depicted on selected cards in the picture booklet used in this test. (This booklet was also used in test J.) On the same sheet was given a list of ten qualities intended to cover a wide range of aspects of personality. In addition, brief explanations of those qualities were given. These explanations, it appeared, turned out to be somewhat more difficult of comprehension than was at first anticipated. The grading of qualities was to be done according to the three-point scale given at the foot of the Explanatory Sheet, such grading being deemed fine enough for the material employed. Each subject was given a prepared blank on which to record his gradings. A copy of the Explanatory Sheet appears below.

Explanatory sheet

1. Simplicity—Applied to a person who seldom sees when he is being tricked; unsuspecting; credulous.
 2. Will-power—Power to force oneself to do disagreeable duties.
 3. Self-control—Power to hold oneself back from doing something that is very pleasant but wrong.
 4. Tendency to Cheerfulness—Applied to one who is usually cheerful, and regains the cheerful mood very quickly after hearing sad news.
 5. Tendency to Anger—Applied to a man who is very easily angered, even by trivial things.
 6. Honesty—Will not cheat or tell lies.
 7. Suggestibility—Easily led to believe things that are obviously untrue, and to do things that are even silly.
 8. Kindness.
 9. Self-display—Love of showing oneself off; love of being looked at; love of attracting attention.
 10. Self-esteem—Has a very high opinion of himself or herself.
- Put a $\times$ if you think the person has "VERY MUCH" of any of these qualities mentioned above.
Put an 0 if you think the person has just a medium amount or average amount of any of these qualities.
Put a — if you think the person has "VERY LITTLE" of any of these qualities.

Test 5 (L). The fifth test was similar to the first two of this series, as regards the making of the key and the conduct of the test. The materials used in this test were, however, photographs of well-known actors in psychological situations.

Test 6 (M). The material for this test was obtained from a stock of personalities whom the present author has known well in real life. The key was prepared in the same way as in the previous tests.

This test was conducted in the same way as test K, except that the personalities referred to were described verbally instead of pictorially. The ten qualities of personality, the Explanatory Sheet, and the three-point scale were those used in test K. In each verbal description clues were given as to dominant qualities. Below is a copy of the first description in the test.

Annie

Annie is of medium height, slim, has a roundish face and clear delicate skin. She has just turned seventeen and her friends find she is becoming very pretty.

At her High School she works hard and has taken one or two prizes in the English, Latin and German classes. Outside school she has no girl friends, but goes about with her mother and elder sister, aged nineteen and also a very pretty girl.

A short time ago, her father managed to buy a motor-car on the instalment plan, and Annie gets him to drive her to school so that she should reach the school gate at about 8.50 a.m., just before the school children go in.

Recently a friend of hers reminded her that only a year ago her father could not afford to buy a car and had driven Annie round in his motor-van. To this Annie replied: "Oh, he could afford it then, but he drove the van for practice in driving."

No time limit was set for any of these tests, but it was found that 40 min. was ample even for the slowest subject.

Test 7 (N). The aim of this test was a measure of ability to discriminate between true and false utterances. The preparation of this test involved the solution of a problem relating to the employment of actual persons as test 'material'.

Even when individuals consent to utter falsehoods for experimental purposes, there still remain difficulties. In a preliminary series of experiments which the present writer had devised and tested he found from a consideration of the data, obtained both from the remarks of the assessors and the introspections of the assessed, that as soon as the individual became accustomed to the experimental procedure, he ceased to be conscious of any deception on his part. Goldstein (5) has shown that manifestations of falsehood are to be observed only when a subject is conscious of an attempt to deceive. If he is not aware of the deception, then what he says will appear to be spoken truthfully.

The difficulty of remaining conscious of deception is, as a rule, very great for a subject in laboratory conditions. Then, again, some subjects become disturbed and speak truthfully instead of following the directions of the experimenter. Further, the host of monosyllabic answers were found much too difficult to interpret by even the mature students and graduates who acted as judges in the tests. A final difficulty was encountered in that statistics for each small group had to be calculated separately; for a truth or a falsehood, once uttered, could not be repeated by the individual for another group. A few judges in a small group of, say, thirty could cause a large disturbance to correlations.

To obviate all these difficulties I employed the following procedure. I obtained the services of a capable adult, M, who was able to act as subject for the assessors as required. Further, I controlled the utterances of M; that is, instead of leaving them to the mercy of chance reproduction and emotional disturbances other than from the stimulus employed in the test item, I decided beforehand not only the utterances but also the concomitants of M's responses. 'Artificial' situations were prepared in place of 'real' ones. These 'artificial' situations used as bases the data obtained from the introspections in the truth and falsehood test referred to above. With these data I prepared a set of eighty questions of a personal kind referring to M. Forty of these required true, the other forty false, answers.

Concomitants of truthfulness and falsehood were selected from the data already obtained, and arranged to accompany the responses of M. These concomitants consisted of facial expressions, modulations of the voice, emphasis on words or syllables, pauses, etc., such as would usually be met with in the simpler and more demonstrative types of people whom we encounter in our daily life. M practised these until he was thoroughly proficient in their usage, when I deemed him fit to act before the assessors in the actual experiment. For test purposes an assistant was procured to ask the questions of M.

Prefacing this test M read out to the subjects, in explanation of his behaviour, the following paragraph: "I am going to imitate various people's ways of telling the truth and telling falsehood, and you will write down whether you think I am representing a truth or a falsehood. Put a cross if you think I am imitating a truth. Put a nought if you think I am imitating a falsehood."

Test 8 (O). In the last of this series of tests I employed four 'problematical situations' similar to Webb's (6) two, and one of his own, making five in all. Below is a copy of one of the questions in this test.

Imagine the following situation:

Mrs R is a poor but very proud woman who was once wealthy. Her husband is dead and her three children are all of school age, the eldest being eleven. She needs help in the way of food, for she is not strong enough to earn sufficient for her children and herself. You want to give her some money, but if you try to give her anything as a gift it will be refused.

(1) What would you do so that she must accept the money you give her?

(2) State your reasons for taking the course that you take.

(2) *Reference values*

Two factors, it seemed, could be controlled in this experimental work; the first of these was 'g'. With this aim I used the following three non-verbal tests: "Analogies" (test E), "Rights" (test F), "Alphabetical Series" (test G).

(i) *Non-verbal tests*

Test E. This consisted of 24 items in each of which a set of three geometrical figures were presented along with a blank. The subject was required to make the second pair analogous to the first by filling the blank with a figure selected from an accompanying set of six other figures. It was deemed advisable, for the sake of clearness, to preface this test with samples of both verbal and non-verbal specimen analogies.

Test F. The 22 items of this test, also employing geometrical figures, were presented as follows. On the left-hand side of each page were two short columns of figures or designs headed by the words "Right" and "Wrong" respectively. The subjects were required to state the numbers of the "Right" figures on the opposite half of the page. Two examples were demonstrated before the subjects were started on the test.

Test G. This test consisted of 20 items, each containing a repeated pattern of letters, followed by four blanks which the subjects had to fill by continuing with the appropriate pattern. The test items were arranged in order of difficulty. They were introduced with six samples worked out for the subjects.

Since the first eight tests here described involved the use of words I decided to add as controls the following four verbal tests: (A) Understanding Paragraphs; (B) Classification; (C) Disarranged Sentences; (D) Proverbs.

(ii) *Verbal tests*

Test A. This was presented in the form of a booklet each page of which contained a short paragraph, e.g. 'Hints on Pastry Making', and a questionnaire referring to it. The subjects were to read the paragraph, more than once if necessary, and write the answers on the blank sheets accompanying the booklet. The test contained 29 questions.

Test B. This was based on Spearman's *A Measure of Intelligence*, and consisted of 48 items. In each the subjects were required either to select and underline one of four accompanying words that was synonymous with, or antonymous to, a given word; or to treat similarly one of four words that gave the best answer to a question introducing them. Two specimen items preceded the test.

Test C. In this test, which consisted of 28 items, the subjects were required to rearrange the jumbled words, or the nonsensical order, of each item to make a sensible sentence. The test was prefaced with two examples.

Test D. The 25 items of this test were introduced with one example. The subjects were required to choose, out of a group of four proverbs, two that meant most nearly the same as the one that headed the item.

The above 'Reference Value' tests, with the exception of B, were supplied by Stephenson(7).

III. EXAMINATION OF THE DATA

(1) *General Results*

This battery of fifteen tests was applied to 203 girls, aged between $13\frac{1}{2}$ and 14 years, attending secondary schools in the London area. The seven schools tested may be considered representative of such schools in general. The tests were administered to groups of not more than thirty at a time. The time allowance appears to have been ample, many of the subjects completing the tests appreciably before the limit.

Tests C and D, especially the latter, I considered might involve a peculiar ability, or abilities, closely related to such as might appear in my own tests (H, I, J, K, L, M, N, O).

For the population of 203 the scatter of scores for each test was approximately of Normal Probability Curve shape. Where necessary, they were reasonably scaled so that scores finally ranged from 0 to about 15, the mean being about 8.

Few anomalies appeared, either in testing, scoring, scaling, or correlation. Only tests K and M showed disturbances in the scoring, attributable, as I subsequently learned, to the fact that a very complicated rating scheme was involved in both.

For convenience of reference, the tests are summarized in the Appendix.

(2) *Examination of the data for ‘g’ factor*

Table 1 shows the inter-correlations¹ for the fifteen tests used in this research. The letters A, B, C, etc. refer to the tests listed in the Appendix.

For convenience the non-verbal tests E, F, G will be referred to as ‘*n*’ tests, the verbal tests A, B, C, D as ‘*v*’ tests, and my own as ‘*ψ*’ tests.

Table 1. *Product-moment correlations for 203 subjects*

	B	C	D	E	F	G	H	I	J	K	L	M	N	O
A	324	327	409	221	131	172	130	120	308	060	089	085	143	183
B	—	376	414	242	141	073	197	146	371	216	170	195	027	198
C	376	—	628	440	377	452	389	376	507	223	329	330	244	309
D	414	628	—	414	298	342	440	404	562	342	339	419	281	314
E	242	440	414	—	489	440	205	218	212	135	135	224	153	091
F	141	377	298	489	—	473	118	195	247	098	129	113	192	097
G	073	452	342	440	473	—	266	265	240	128	219	241	135	133
H	197	389	440	205	118	266	—	509	526	094	288	177	156	247
I	146	376	404	218	196	265	509	—	381	156	414	562	219	220
J	371	507	562	212	247	240	526	381	—	381	362	495	320	352
K	216	223	342	135	098	128	094	156	381	—	173	196	130	172
L	170	329	339	135	129	219	288	414	362	173	—	317	090	132
M	196	330	419	224	113	241	177	562	495	196	317	—	176	280
N	027	244	281	153	192	135	156	219	320	130	090	176	—	113
O	198	309	314	091	097	133	247	220	352	172	132	280	113	—

It will be seen that all the correlations are positive and some are quite high. Table 2, summarizing Table 1, shows the mean correlation of the ‘*n*’ and ‘*v*’ with the ‘*ψ*’ tests.

Table 2.

Mean correlation of A with the ‘ <i>ψ</i> ’ tests				0.121
”	”	B	”	0.178
”	”	C	”	0.338
”	”	D	”	0.439
”	”	E	”	0.172
”	”	F	”	0.148
”	”	G	”	0.203

It is of immediate interest that the tests A, B, E, F, and G correlate very little with the ‘*ψ*’ tests, whereas C and D, especially the latter, correlate more highly with the ‘*ψ*’ tests.

There are available correlations for the three non-verbal tests E, F, G. These tests were expected to supply a measure of the fundamental common factor ‘*g*’ upon which this factor study has been based. Stephenson(7) has shown that such non-verbal tests fit the tetrad difference criterion for a population of one thousand, and tests E and G were actually those used in that work.

With three tests only tetrad differences cannot be constructed, but by taking the above three ‘*n*’ tests together with one of the ‘*v*’ or ‘*ψ*’ tests singly, tetrads of the following type can be constructed:

$$r_{n_1n_2}r_{n_3v} - r_{n_1n_3}r_{n_2v} = f_i. \qquad \text{Type (i)}$$

There are two other tetrad differences for the four variables:

$$r_{n_1n_2}r_{n_3v} - r_{n_2n_3}r_{n_1v} = f_{ii}, \qquad \text{Type (ii)}$$

$$r_{n_1n_3}r_{n_2v} - r_{n_2n_3}r_{n_1v} = f_{iii}. \qquad \text{Type (iii)}$$

Further, these are critical for two important facts: (a) for ‘*g*’ factor throughout the ‘*n*’ tests; (b) for the same ‘*g*’ factor running through all the other (‘*v*’ and ‘*ψ*’) tests.

If tetrads of Type(i) satisfy the tetrad criterion then there is evidence for a common factor ‘*g*’ that runs through all the fifteen tests, verbal and ‘*ψ*’ as well as non-verbal.

¹ Correlated by the Method of Differences. See Kelley (T. L.), *Statistical Method*, p. 180, Formula 134.

There are 36 tetrads of Type(i) for Table 2. These supply the following data:¹

Observed mean of 36 tetrad differences	0.0313
Observed mean $\times 0.84$	0.0264
Theoretical probable error ²	0.0220

Observation and theory must be considered to fit very well. Actually, although the observed probable error is slightly greater than the theoretical one, no single tetrad difference is more than 3.5 times the theoretical probable error.

Further, it is considered that a 'spurious' anomaly³ must explain the very low correlation, 0.073, between the two 'intelligence' tests, Alphabet Series and Classification (G and B); whilst the correlation between F and M (0.113) is similarly undoubtedly a value that a repeated testing would raise. These two correlations r_{BG} and r_{FM} , in fact, result in the largest tetrads amongst our thirty-six under consideration: their removal is sufficient to bring the observed probable error down to a mean value of 0.0212 (for the theoretical probable error 0.0220).

Whether these two correlations are allowed or not, it is important to note that none of the r_{nn} correlations involves 'specificity' ('specific correlations' or 'group factor(s)') relative to correlations of the type r_{nv} or $r_{n\psi}$.

Thus the critical tetrads above fit the tetrad criterion extremely satisfactorily: a 'g' factor is demonstrated for the non-verbal tests and this runs through all the tests, verbal and ' ψ '. When two 'n' tests are correlated together no 'specificity' is manifested.⁴

(3) 'v' factor

Previous work with verbal and non-verbal tests (7) shows that verbal tests involve 'specificity' over and above 'g' factor. One must determine, therefore, results for tetrads involving correlations of type r_{vv} critically, with a view to bringing further evidence to bear on the problem of 'v' factor. The tetrads required are those of critical X-form, namely:

$$\left. \begin{aligned} r_{v_1 v_2} \cdot r_{n_1 n_2} - r_{v_1 n_1} \cdot r_{v_2 n_2} &= f_{ii} \\ r_{v_1 v_2} \cdot r_{n_1 n_2} - r_{v_1 n_2} \cdot r_{v_2 n_1} &= f_{ii} \end{aligned} \right\} \quad (X)$$

In these tetrads, it will be observed, r_{vv} occurs only on the left side of the differences; we already know that the correlations r_{nn} and r_{vn} do not involve 'specificity', so that if 'specificity' appears for correlations of the type r_{vv} , tetrads of form X must show significant positive differences.

On the other hand, the third tetrad that can be constructed for the four tests is as follows:

$$r_{v_1 n_1} \cdot r_{v_2 n_2} - r_{v_1 n_2} \cdot r_{v_2 n_1} = f_{iii} \quad (Y)$$

This is called by Stephenson a Y-form tetrad. Even though specificity appears for r_{vv} correlation, the Y-form tetrads might fit the tetrad criterion; from the data given in the

¹ Throughout I follow here the technique described by Stephenson (7) in dealing with the tetrad differences. The observed mean is the mean deviation about the mean (since the mean for any set of tetrads is zero), so that all tetrad differences are deviations from the mean which is zero. The observed mean $\times 0.84$ gives the observed probable error for the data. The observed and theoretical results, then, are those given by this observed probable error and the theoretical probable error (i.e. values 0.0264 and 0.0220 above). I have used only the approximate formula for the theoretical probable error, which, indeed, is sufficiently accurate for the present data.

² Given by Spearman's (8) formula 14.

³ I understand from Stephenson (7) that correlations that are too low are rarely met with in tetrad examination; if they are met, their explanation is found in non-linearity of the correlations or other influences that have a misleading effect on matters relating to the psychological factors under consideration.

⁴ It will be observed that nothing has yet been said concerning the factor content when two 'v's or two ' ψ 's are together in a tetrad.

section above dealing with 'g' factor, indeed, these Y-form tetrads must fit the criterion if 'g' runs through all 'v' and 'n' tests. Tetrads of a very critical form, namely Z-form, of type

$$r_{n_1 n_2} \cdot r_{n_3 v} - r_{n_1 n_3} \cdot r_{n_2 v} = f$$

were found previously to fit the tetrad difference criterion.

Considering the data for these two forms of tetrads, we have results as follows. There are thirty-six X-form tetrads for the three 'n' and four 'v' tests. These have means, etc. given below:

Observed mean of thirty-six X-form tetrads	0.1215
Observed mean $\times 0.84$	0.1020
Theoretical probable error	0.0180

Every X-form tetrad difference is positive.

The eighteen Y-form tetrads, on the other hand, give:

Observed mean	0.0227
Observed mean $\times 0.84$	0.0220
Theoretical probable error	0.0180

That is, the Y-form tetrads fit the criterion extremely well (no tetrad difference is four times the probable error), and the largest values, differences of 0.0772 and 0.0525 respectively, are attributable to the two 'spuriously' low correlations r_{BG} and r_{FM} . With allowance for these two tetrads the observed mean sensibly approaches the theoretical probable error.

Secondly, the X-form tetrads are all positive and of a mean amount about six times the theoretical probable error, some differences ranging as high as ten times the probable error.

My conclusions, then, proceed as follows:

(a) 'g' factor has been found to run through all the tests, verbal and non-verbal alike (previous section).

(b) The Y-form tetrads fit with the result (a).

(c) Thus, in considering the X-form tetrads, where marked differences are found, I must attribute the disturbances to correlations of the type r_{vv} , for, by (a) and (b) above, correlations of the type r_{nn} and r_{nv} do not involve 'specificity'.

I may, then, look for a group factor, or factors, in the 'v' tests over and above their common 'g' factor content.

'g' saturation of the tests. Before proceeding to partial out 'g' factor from the correlations, it is well to report here the 'g' saturation of the various tests. For any test, 'g' saturation is given by the equation:

$$r_{ag}^2 = \frac{r_{ab} \cdot r_{ac}}{r_{bc}}$$

when *b* and *c* are 'reference' tests.

In my work, the 'reference' tests are the three non-verbal tests for which 'g' factor has been demonstrated.

Table 3 shows the 'g' saturation for all fifteen tests.

Whilst the reference tests have highest 'g' saturation, it must be observed that tests of Disarranged Sentences (C) and Proverbs (D) are scarcely less well saturated with 'g' factor.

It may seem surprising that the Verbal Classification shows so little 'g' saturation, but the Paragraphs test (A) shows equally little; and, as will be seen later, the result is in keeping with a theory of 'v' factor.

Table 3.

Test	'g' saturation
Alphabetic Series	0.653
Analogies	0.675
'Rights'	0.725
Disarranged Sentences	0.617
Understanding Paragraphs	0.252
Proverbs	0.512
Classification (Verbal)	0.211
' ψ ' test: H	0.281
I	0.329
J	0.340
K	0.175
L	0.232
M	0.276
N	0.234
O	0.155

Specific correlations with 'g' factor partialled out. By use of the well-known formula given by Spearman (8), Appendix, p. xvi, formula 19), 'g' factor may be partialled out of all the tests. The formula is as follows:

$$r_{ab.g} = \frac{r_{ab} - r_{ab}.r_{ab}.r_{bg}}{\sqrt{(1-v_{ag}^2)}\sqrt{(1-v_{bg}^2)}},$$

where a and b are any two tests used.

Obviously, the correlations of type r_{nn} give no specific correlation: their mean, when 'g' is partialled out, is zero. Table 4 shows the specific correlation for the remaining verbal and ' ψ ' tests, the 'g' saturations given in Table 3 being used in the calculation. It is observed that all the correlations are still positive.

The letters A, B, etc. refer to the tests listed in the Appendix.

Table 4. *Specific correlations for 203 subjects*
(Derived from Table 1, 'g' being partialled from Table 1)

	B	C	D	H	I	J	K	L	M	N	O
A	289	228	338	064	141	245	017	033	017	090	152
B	—	320	364	147	084	325	187	126	145	023	170
C	320	—	255	287	234	404	149	244	212	131	275
D	364	255	—	360	292	480	300	264	338	194	276
H	147	287	360	—	460	477	048	239	108	096	215
I	084	234	292	460	—	303	112	368	519	154	180
J	325	404	480	377	303	—	350	311	445	262	323
K	187	149	300	048	112	350	—	150	157	093	150
L	126	244	264	239	368	311	150	—	271	038	100
M	145	212	338	108	519	445	157	271	—	118	270
N	023	131	194	096	154	262	093	038	118	—	080
O	170	275	276	215	180	323	150	100	270	080	—

For the four verbal tests the mean of the correlations with all the ' ψ ' tests is given in Table 5.

Table 5

Mean for Paragraphs test with all ' ψ ' tests	0.082
Mean for Verbal Classification with all ' ψ ' tests	0.145
Mean for Disarranged Sentences with all ' ψ ' tests	0.242
Mean for Proverbs test with all ' ψ ' tests	0.313

It will be observed from Table 5 that tests D(Proverbs) and C (Disarranged Sentences) correlate most highly with the ' ψ ' tests. An interpretation of this fact will be given in the later section of this present work.

Let us now turn to tetrad examination of the material. There are only four verbal tests, A, B, C, D. The three tetrads for the intercorrelation for these tests have the mean value 0.0227 (i.e. observed probable error 0.0190), for a theoretical probable error 0.0200. Thus, slight as the data are, they are in keeping with a theory of a common 'v' factor.

Of more importance, however, is the question whether a common factor can be taken to run through all the verbal and ' ψ ' tests, i.e. throughout Table 4. Table 4, as a whole, however, does not satisfy the tetrad difference criterion: critical tetrads involving r_{HI} , for example, have a mean value of about 0.140 for a probable error 0.020, and even after removal of several such correlations, the whole table is not completely satisfied by the tetrad difference criterion. Tests K and M, each of which involves the use of an admittedly complicated rating scheme, give large tetrad disturbances: there is surely something anomalous or 'spurious' about a test that correlates 0.52 with test I, and 0.11 with test H, when H and I themselves correlate 0.46. Non-linear correlation or misunderstanding of test requirements leading to anomalous score distributions might be considered sufficient to explain these unsatisfactory correlations for tests K and M. Many of the subjects, I learned subsequent to the testing, had difficulty in the symbolic use of the plus and minus signs required in these tests. For the present, then, I propose to ignore tests K and M, and add the opinion that, had more time been spent in explaining the requirements in the rating scheme, then these tests would have correlated like the other ' ψ ' tests.

The statistical treatment could now be pursued as follows. On the one hand, I could follow out the hypothesis of a single common factor that is taken to run through all tests in Table 4 (omitting K and M): Any disturbances (i.e. large tetrad differences) would later be examined for psychological explanation.

On the other hand, I could apply a statistical criterion to the theory of a 'v' factor that runs through the verbal tests; it must enter, too, into the ' ψ ' tests, where words and phrases may be a very important feature of the tests.

But these methods are, in effect, identical, though the second allows a clearer approach to the problem. 'v' factor has been sufficiently demonstrated by Stephenson (7), and the two tests A and B were intended primarily to offer the means for controlling the factor. It is true that all four verbal tests fit the tetrad criterion; but only the Proverbs test might be expected to involve very highly any specific factor that might also be involved in the ' ψ ' tests, over and above 'v' factor, so that the influence of such a factor would scarcely be expected to show when taken with the other 'v' tests—even with test C (Disarranged Sentences). In any case, I have considered worth testing a theory that 'v' factor is entailed only in tests A and B. In terms of these two tests, then, 'v' factor can be partialled out from all the other tests in Table 3.

'v' saturation of tests. Using the tests A and B as 'reference' values, the 'v' saturation for any test, say C, is given by

$$r_{av}^2 = \frac{r_{ba}r_{ca}}{r_{bc}}.$$

I have given in Table 6 the 'v' saturation of the various tests calculated from Table 4 (i.e. with 'g' already partialled out).

One may ask, before continuing, whether the above 'v' saturations are reasonable values for the tests involved. The verbal tests, as a whole, have a high 'v' saturation. The only other tests holding a similar high degree of 'v' factor are J and O. An examination of the tests J and O shows that J has by far the most complicated 'verbal' content of any of

the ' ψ ' tests, whilst O is also highly verbal (paragraphs are to be read). In the other tests verbal factors cannot be taken as at all critical; the language is very simple, and the critical matter is undoubtedly not the meanings of the phrases or words. I conclude, then, that where I should expect high ' v ' factor, there and only there does it occur. The procedure adopted seems justified.

Table 6

Test	
A. Understanding Paragraphs	0.454
B. Classification (Verbal)	0.636
C. Disarranged Sentences	0.503
D. Proverbs	0.652
H. 'Expressions' (Pictorial)	0.181
I. 'Situations' (Pictorial)	0.138
J. 'Character Qualities' (Selective)	0.525
L. 'Situations' (Photographic)	0.120
M. 'Verbal Delineations'	0.092
N. 'Truth and Falsehood' (Discrimination)	0.085
O. 'Problematic Situations'	0.293

(4) *A final correlation table, with ' v ' factor partialled out*

Using the above ' v ' saturations one can finally calculate new specific correlations for Table 4. These values in Table 4 already have ' g ' partialled out; but we can repeat the process for them, and partial out ' v ' factor. Thus, the specific correlation for tests C and D is given by the formula:

$$r_{cd.v} = \frac{r_{cd} - r_{cv}r_{dv}}{\sqrt{(1 - r_{cv}^2)}\sqrt{(1 - r_{dv}^2)}},$$

and similarly for all values CH, CI, CJ, ..., etc.

Table 7 shows the specific correlation remaining after the ' v ' factor has been partialled out. Tests A and B have been omitted for clearness; their mean correlations with all the other tests are 0.01 and 0.00 respectively.

Table 7. *Specific correlations for ' g ' and ' v ' partialled out*

N=203. Specific correlations derived from Table 4.

	C	D	H	I	J	K	L	M	N	O
C	—	206	230	192	190	111	213	193	103	145
D	206	—	324	266	212	305	246	370	194	114
H	230	324	—	446	410	030	223	093	084	174
I	192	266	446	—	272	100	356	515	144	147
J	190	212	410	272	—	350	317	467	255	203
K	111	305	030	100	350	—	139	149	085	125
L	213	246	222	356	317	139	—	263	030	068
M	193	370	093	515	467	149	263	—	112	255
N	103	194	084	144	255	085	030	112	—	058
O	145	114	174	147	203	125	068	255	058	—

It is immediately striking that all the correlations in Table 7 are positive. Some values, indeed, are very large (0.446, 0.410). Now, with ' g ' and ' v ' partialled out, we have at last arrived at correlations that might give evidence of a third factor.

Tetrad examination of Table 7. The problem of a third factor can be answered only after the application of the tetrad difference criterion. With this in view, I shall, therefore, proceed to examine Table 7.

Omitting tests K and M for the reasons mentioned above, I am left with the following tests:

- C. Disarranged Sentences.
- D. Proverbs.
- H. 'Expressions' (Pictorial).
- I. 'Situations' (Pictorial).
- J. 'Character Qualities' (Selective).
- L. 'Situations' (Photographic).
- N. 'Truth and Falsehood' (Discrimination).
- O. 'Problematic Situations'.

There are 210 tetrad differences for the intercorrelations of the above eight tests. These supply the following data:

Observed mean of 210 tetrads	0.0201
Observed mean $\times 0.845$	0.0170
Theoretical probable error	0.0150

The agreement between theory and observation is very close. Further, by removing tetrads involving r_{IJ} and r_{LN} the observed mean becomes 0.0153, i.e. almost exactly the theoretical probable error. Both values r_{IJ} and r_{LN} are apparently too *low*—facts that are never substantiated with repeated testing; i.e. some testing anomaly or fault is probably at the basis of these low values. The important fact is that no single correlation can be isolated that is too *big*.

Thus we must conclude that the above tests fit the tetrad criterion satisfactorily. A new factor is demonstrated: it may be named ' ψ ' factor for the present.

(5) *Saturations or loadings with 'g', 'v', ' ψ '*

Finally, before turning to explain the ' ψ ' factor (for the statistical procedure simply isolates the factor, and its explanation in psychological terms must be looked for elsewhere), it will be of interest to look at the ' g ', ' v ', and ' ψ ' saturations for the various tests. The ' g ' and ' v ' saturations are given in previous tables, and from Table 7, the ' ψ ' saturation can be calculated from each of the tests C, D, H, I, J, L, N, O. Table 8 shows this composite table of saturations.

One can see from Table 8, in concise form, the essential conclusions as regards the factors which have emerged in my work. These conclusions may be summarized in the following way:

With regard to 'g' factor: (a) The non-verbal tests are highly saturated with ' g ' and have neither ' v ' nor ' ψ ' saturation.

(b) The verbal tests C and D are also highly saturated with ' g '.

(c) Tests A and B and the ' ψ ' tests have relatively poor ' g ' saturation.

With regard to 'v' factor: (a) All the verbal 'intelligence' tests (A, B, C and D) are fairly highly saturated with ' v ' factor.

(b) Tests A, B and J are significantly more highly saturated with ' v ' factor than with ' g ' factor.

(c) Tests C and D are as highly ' v ' saturated as they are ' g ' saturated.

(d) Except for test J all my ' ψ ' tests are poorly saturated with ' v ' factor.

- With regard to 'ψ' factor:* (a) The four pictorial tests H, I, J and L are highly 'ψ' saturated; they are more saturated with 'ψ' than with 'g' or 'v'.
- (b) The verbal tests C, D and O are less well 'ψ' saturated.
- (c) The four tests H, I, J and L are almost as highly 'ψ' saturated as any of the tests are 'g' or 'v' saturated.

From the above data, tests H, I, J and L must be regarded as providing almost as good a measure of 'ψ' as the non-verbal tests provide a measure of 'g'.

Table 8. *Showing 'g', 'v' and 'ψ' saturation for the various tests used in this work*

	'g'	'v'	'ψ'
A. Understanding Paragraphs	252	454	Nil
B. Classification (Verbal)	211	636	Nil
C. Disarranged Sentences	617	503	390
D. Proverbs	512	652	490
E. Non-Verbal Analogies	675	Nil	Nil
F. Non-Verbal 'Rights'	725	Nil	Nil
G. Alphabetical Series	653	Nil	Nil
H. 'Expressions' (Pictorial)	281	181	614
I. 'Situations' (Pictorial)	329	138	590
J. 'Character Qualities' (Selective)	340	525	606
K. 'Character Qualities' (Grading)	175	105	—
L. 'Situations' (Photographic)	232	120	442
M. 'Verbal Delineations'	276	092	—
N. 'Truth and Falsehood' (Discrimination)	234	085	246
O. 'Problematic Situations'	155	298	263

IV. INTERPRETATION OF THE FACTORS

'g'. An attempt might be made to explain 'g' factor (9) in terms of those relations which are actually involved in the non-verbal tests, i.e. excluding the psychological relation. But the evidence brought forward hitherto by research workers in this field points to the conclusion that, on the whole, the factor 'g' would not appear to characterize any relation in particular, but enters about equally into mental processes involving eduction(8) of relations of any kind. There is no need, therefore, to attempt an explanation of either 'v' or 'ψ' in terms of ability to deal with a special relation, e.g. the psychological one, which might have been expected to be chiefly involved in the 'ψ' tests.

'v' and 'ψ'. To explain 'v' factor Stephenson(7) has advanced a theory which describes this in terms of two components: (a) an ability to supply 'meaning' at the stimulus of a word, phrase or sentence; (b) an ability to produce a correct (i.e. descriptive of the fact) word, phrase or sentence when a meaning is already in one's mind; for one may have a meaning in mind and yet be at a loss for a word to describe it. Stephenson's theory may be expressed otherwise as follows: to evoke a symbol to describe an idea or notion is to be distinguished from 'understanding' the symbol when this is presented to the mind. The two components of 'v' may be quite disparate.

The 'v' factor in the tests here described might then be identical with Stephenson's (a) component, while my 'ψ' factor might be identical with his (b) component.

Applying this hypothesis to the tests used in this research, we find that in test A (Understanding Paragraphs) and B (Verbal Classification), or in any test or situation where written or spoken words are used, the first component might be expected to function. In both the above tests, it will be remembered, the subjects were allowed only a short time, so that, apart from the eduction of relations, the factor (a) might be critical. The tests which Stephenson used in his own research on 'v' factor were, in great part, of the 'Understanding Paragraphs' type, so that (a) factor would operate largely.

The ' ψ ' factor functions in high degree in the pictorial tests, but the Proverbs, Disarranged Sentences, Problematical Situations, and Truth and Falsehood tests all have some measure of this factor. In the verbal portion of these tests does the suggested ability, component (*b*), enter? That is, when meanings or ideas are already in the mind, is the ability to find words for expressing those the critical one? In the Proverbs test, for example, one may suppose that the ability to deal with the idea roused by the given proverb (and by each of the other four proverbs from which the selection is required) is measured by ' v ' factor; and that this idea being in the mind, the further arrival at the appropriate word, phrase, or sentence is critical. But the appropriate (along with the less appropriate or inappropriate) sentence is supplied for the subject's selection. Stephenson's (*b*) component, therefore, cannot be accepted as functioning critically in such a test as Proverbs, and we must turn elsewhere for an explanation of ' ψ ' factor.

Since this occurs in some verbal but in no non-verbal tests, an examination of the functions of language was considered necessary with a view to throwing light on both ' v ' and ' ψ ', more particularly on the latter.

Regarding the origin of language, Jespersen⁽¹⁰⁾, agreeing with Plato⁽¹¹⁾ who points to the relation between animal cries and human speech, suggests especially those primitive social situations in which a highly emotional state was reached. In keeping with such an emotive theory of its origin, Pillsbury & Meader⁽¹²⁾ employ the term 'language' to denote all expressive movements of all muscles and also all expressive secretions. According to these authorities, language is, in the main, muscular movement, either voluntary or involuntary "organically determined by changes in the nervous system of the communicator, and capable of being appreciated as significant by another individual". The wide opening of the eyes and mouth together with the contraction of the vocal cords and escape of the breath as a cry (in the state of terror), the blush, the sigh, indicative of the quality and intensity of the experienced emotion, would all come under the head of involuntary expressive movements.

Voluntary movements are divided into two classes; the first aim at being communicative, the second are not so intended⁽¹²⁾. As an example of the latter may be cited the rapid walk or run of an individual. In its proper setting such movement may convey information of great importance regarding the mental state of that individual. Another illustration may be taken from the legal sphere; in a court, slight unintentional movements may reveal the very truth which a witness intends to cloak with words.

It may be observed here that 'meaning' in the sense of "that which the speaker intends the listener to refer to" is to be distinguished, as Ogden & Richards⁽¹³⁾ point out, from 'meaning' in the sense of "that which the speaker intends the listener to feel a certain attitude to, to have certain feelings towards, to make a response to, to suppose the speaker is desiring", etc.

The first of these meanings, namely, "that which the speaker intends the listener to refer to", is concerned especially in deception. When a deceiver is successful with a falsehood, the victim makes the reference which the former intends he shall. In this case, he must be considered to have grasped the deceiver's 'meaning'. But a shrewder interpreter may either reject the intended reference, or arrive at one quite different from that intended by the would-be deceiver. If, by taking into account the total behaviour of the speaker (including his words) he has divined his true, ulterior intention, the interpreter may be said to have reached the would-be deceiver's 'true meaning'. Since this last was not intended

to be grasped by the listener, this meaning may be termed 'non-symbolic' or 'non-representational'.

The batsman who correctly plays a 'googly' is making exactly the same kind of interpretation. He reaches the 'true meaning' of the bowler's action by discounting certain of the signs exhibited. The 'googly' bowler does not intend that all his movements should be communicative. On the contrary, he strives to prevent them from being so; yet, to certain observers, in spite of all his volition and inhibitive power, those very movements which he seeks to hide are communicative *though not intended to be so*. These latter are 'non-symbolic'; only some of his overt movements, the symbolic ones, are designed to be communicated, to be 'understood' as his intention.

These two functions of language, the referential or purely symbolic, and the affective-conative or non-symbolic, recall the distinction made by Aristotle between 'enunciative' on the one hand and interrogative, precatory, imperative, on the other⁽¹⁴⁾.

Some words may be purely symbolic to many people, yet to others these same words may have an affective-conative value, e.g. (in modern times) 'house', 'bed', 'chair', where a need is involved.

Miller⁽¹⁵⁾ points out that for a very young child, even the word 'ball' has an affective substratum, because the thing it denotes gives him the *agreeable experience* of causing it to roll. Rignano⁽¹⁶⁾ would say that such a classification of all round things is not of a perceptual order, but of an affective one; the child puts into a class all those things which affect him equivalently. Even adults tend to classify similarly. According to Rignano, in man's early history an entirely similar affective classification was made; the first names were common names, phonetic symbols which indicated classes of objects or phenomena equivalent in relation to a given affectivity, a given need, or a given desire.

Sacred and secret names show this affective function very markedly. Examples of the former may be obtained from almost any religion: the true and great name of Allah, of Brahman, of JHVH is too sacred, too powerfully emotive, to utter in the ordinary way—a less emotive term, 'Adonai' (the Lord) is substituted for the more affective Tetragrammaton, JHVH (Jehovah). Of secret names, excellent illustrations can be taken from ancient Egypt: the Ran, or Great Name, of each person was divulged only to parents and the priest, for it carried very great power over its possessor, who, in everyday life, used only his Little Name. Lovers often have secret names and words with high affective value.

"The extent to which primitive attitudes towards words are still exploited by the astute," write Ogden & Richards, "is fully revealed only when the achievements of some cynical rhetorician are accorded the limelight of the law courts, or when some particularly glaring absurdity is substituted for the more patient methods of suggestion favoured by repetitive journalism"⁽¹³⁾.

The non-symbolic function of language is further subdivided by Vendryes⁽¹⁷⁾ into the 'active' and 'affective'. 'Active' language, though it has borrowed certain ready-made grammatical terms from logical language, "has a role of its own, which, however, remains yet to be studied". It seems to have taken origin from the manifestations of vocal or choral collective ceremonies. "The constituent elements of the cry or song thus took on a symbolic value which each individual kept for his own personal use"⁽¹⁷⁾. The symbol used by one member of a social group could be used to stimulate other members of the group.

From this remote origin is derived the imperative of the verb and that special form of the noun known as Vocative, which, in Latin and Greek, was regularly distinguished from the

Nominative. In English such distinction is expressed, nowadays, by intonation in speech, or by the addition of the interjection 'O' in written language. The congenerous nature of the imperative form of the verb and the instinctive cry may perhaps be better appreciated from a study of an interjection like 'Ssh!' employed to obtain silence.

A simple illustration of active language is to be found in the use of the Latin 'vel' which came to mean the equivalent of our 'or'. 'Vel', it will be remembered, was originally the imperative singular of 'volo' ('I wish'). The affective-conative character of the word in its earliest usages is obvious.

Even simple sentences, as "John struck the dog", may have an affective tinge. But this is a problem, urges Vendryes, for the psychologist studying the nature of feelings, and adds, "also for the artist seeking to reproduce them". The linguist is concerned with these only in so far as they are expressed *by means of language*. There are cases in which these affective expressions "mingle with the grammatical form to the extent of modifying it".

Affectivity in language may be expressed in two main ways: by the choice of words, and by the position they occupy in the sentence, i.e. the principal resources of affective language are *vocabulary* and *syntax* (17).

In illustration of the first method may be cited invective, as instanced, e.g. in the Plautine comedies. Of these, Wedeck (18) writes: "whose characters seem to take a zestful relish in searching for and inventing the most telling, the most extreme and outrageous epithets and pejoratives." The variety of terms used by Plautus to denote, for example, a scoundrel is "choice and extensive, just as the modern French argot of the twentieth arrondissement has a selection of over fifty expressions for 'thief'". In Roman comedy, the actors, slaves temporarily freed from their bonds, can give expression to their pent-up emotions, and can bestow on their master or master's son an accumulation of resentment. The term 'scelus' (lit. 'the essence of crime itself') is indicative of the *emotive attitude*, the hostility, of the utterer in those Roman comedies. To its employer, the slave, the non-symbolic function of the word is of far greater value (in that it relieves, to some extent, a need) than its mere symbolic significance.

As regards the second method of expressing affectivity, namely syntax, it appears that word order is of especial importance in the written language, in that it acts, along with vocabulary, as a surrogate for intonation, emphasis and the gestures which accompany the spoken tongue. One of the chief differences, according to Vendryes, between affective and logical language lies in the construction of the sentence. But this difference between the two linguistic functions is more easily seen when the written is compared with the spoken tongue. "In French," says this philologist, "the two are so far removed from each other that a Frenchman never speaks as he writes and rarely writes as he speaks." There is in each case, besides a difference in vocabulary, a difference in the arrangement of the words. The logical order in which the words of the written sentence are introduced and set, is always more or less dislocated in the spoken sentence. Sentences like the following belong to the written language: "Il faut venir vite." "Quant à moi, je n'ai pas le temps de penser à cette affaire." "Cette mère déteste son enfant."¹ But in the spoken tongue, invariably the form would be entirely different: "Venez vite!" "Du temps, voyons, est-ce que j'en ai, moi, pour penser à cette affaire-là!" "Son enfant, mais elle le déteste, cette mère!" (17)²

¹ "It is necessary to come quickly." "I have no time to think about that matter." "This mother hates her child."

² "Come quickly!" "Look at the time, I tell you! Do you think I've got the time to think about that?" "Her child, why, that mother hates it!"

To produce certain emotional effects which spoken language can obtain by manner, tone of voice, emphasis, pauses, a writer may resort to anacoluthon: a word or phrase is placed at the beginning or end of a sentence without being brought into the common syntax, e.g. "The poor man, someone ought to do something for him," or "A wise child, you give him everything he asks for."

Whether we hold with Vendryes in making a threefold classification of words, logical, active and affective, corresponding to the classical tripartite division of mental phenomena into cognition, conation, affection; or with Ogden & Richards, Pillsbury & Meader, and others, in a twofold, symbolic and emotive, is of secondary importance; for both classifications distinguish the logical or purely symbolic from the non-symbolic function. Following the modern tendency to bracket conation and affection under the ancient term *orexis*⁽²²⁾, we may classify the elements of language for psychological purposes into: (a) logical or purely symbolic, (b) emotive, orectic, affective-conative, or non-symbolic.

I am enabled, after this examination of the functions of language, to give an explanation, as a working hypothesis, of the two factors '*v*' and '*ψ*' which emerged on statistical analysis.

In tests A and B (Paragraphs and Verbal Classification) the '*v*' factor was found, but no '*ψ*' factor, while in test C (Disarranged Sentences) and D (Proverbs) a further factor, '*ψ*', was found in addition to '*v*'.

Examination of tests A and B shows these to be of a kind involving very little affective language. The '*v*' factor in these tests may now be explained, hypothetically, as a reproductive factor (cognitive but not eductive), concerned with the ability to evoke purely symbolic or representational elements in language. This theory of '*v*' factor would fit the facts very well: reference to the factor saturation (Table 8) of the tests shows that, apart from the verbal tests (A, B, C, D), very little '*v*' is involved in the other tests, except in the case of J and O, in which two tests the questionnaire led one to expect a somewhat large amount of '*v*' factor.

Regarding test C, the order of words in this is not the logical one, but that of affective language, manifesting anacoluthon. An examination of test D shows that more than one third of the total number of proverbs employed in the test uses either the imperative of the verb, an affective instrument, e.g. "Don't cry over spilt milk", "Look before you leap", "Stick at your job if you want to succeed", etc.; or else suggestion is made, e.g. "There's many a slip 'twixt the cup and the lip", "He who hesitates is lost". In such cases as this latter type of sentence, an imperative, equivalent to "Bear in mind that..." or "Note that..." is implied. The suggestion goes further: it implies that the proverb "refers to you" (the subject), i.e. an affective process is probably set in action.

In all these cases of virtual anacoluthon (test C) and explicit or implicit imperative, affective language is being used; the writer of the proverb is affecting the orectic aspect of the subject's personality.

The same '*ψ*' factor which occurs in tests C and D also occurs in the pictorial tests, in which it is not difficult to appreciate that the factor may well be explained as an emotive one; the subject assumes or tends to assume the emotive aspect of the person observed in the picture as well as of the affective language which completes the test item.

In showing, therefore, by his score in the '*ψ*' test, whether he has high or low 'psychological ability', an individual would, at the same time, reveal another very important aspect of his personality, namely, the degree of affectivity or emotivity; that is, not only

would the test be a measure of his ability to assess the personality of others, but would also be an objective means⁽¹⁹⁾ of demonstrating the orectic dimensions of a subject: breadth in relation to the variety of psychological situations into which he can 'project' himself correctly, depth as manifested by the strength of his response indicated by his selection from the verbal elements of the test.

Individuals with high 'psychological ability' would appear to fit closely Ribot's⁽²⁰⁾ description of the 'Sensitive' type, "who might also be called the affective or emotional, have as their special characteristics the exclusive predominance of sensibility... They are like instruments in a perpetual state of vibration, and their life is for the most part inward... the extreme susceptibility of (their) nervous system to agreeable and disagreeable impressions".

Further corroboration of the above theory of '*v*' and '*ψ*' factors is lent by the introspections of the present writer recorded at the time of devising the tests, and of his two collaborators (one of whom was Stephenson) in checking the key. Specific affectivity aroused by each pictorial item was subsequently interpreted verbally; the prior process was an orectic one. The present writer experienced feelings together with a tendency to imitate the posture portrayed, followed by interpretation, in the form of verbal expression, of those feelings. (His collaborators reported similarly.) Many of these expressions are highly idiomatic, emotive, employ anacoluthon, and are quite comparable with those quoted by Vendryes in illustration of affective language; e.g. in item 17 of test I, sentence (*d*) reads: "My, that's a lovely one!—if only I could get my hands on it!"; or in item 16, "Oh! oh! it hurts dreadfully!"

It is of interest that tests H and I, since they are nearly as highly loaded with '*ψ*' factor as the non-verbal are with '*g*', have been used by Spearman as an adequate measure of 'Psychological Skill' in the Spearman-Holinger Unitary Trait Study⁽²¹⁾.

APPENDIX

	Time allowed for test (min.)	No. of test items	Mean score obtained	Standard deviation
A. Understanding Paragraphs	8	29	6.64	2.27
B. Classification (Verbal)	6	48	5.01	2.43
C. Disarranged Sentences	5	28	4.93	1.98
D. Proverbs	6	25	5.38	1.89
E. Non-Verbal Analogies	5	24	6.35	2.63
F. Non-Verbal 'Rights' (Classification)	8	22	6.68	3.42
G. Alphabetical Series	5	20	7.94	3.23
H. 'Expressions' (Pictorial)	40	40	9.55	3.75
I. 'Situations' (Pictorial)	40	47	6.71	2.43
J. 'Character Qualities' (Selective)	40	75	5.72	1.90
K. 'Character Qualities' (Grading)	40	30	7.18	2.22
L. 'Situations' (Photographic)	40	40	6.50	2.53
M. 'Verbal Delineations'	40	49	10.06	4.28
N. 'Truth and Falsehood' (Discrimination)	40	80	8.63	4.50
O. 'Problematic Situations'	40	5	5.18	2.14

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THE TRANSPOSITION OF VISUAL PATTERNS¹

BY VALERIE DEES AND G. C. GRINDLEY. *From the Cambridge Psychological Laboratory*

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I. INTRODUCTION

If a figure is shown repeatedly, always on the same part of the retina, until it is immediately recognized and discriminated from other figures, will this immediate recognition and discrimination 'transfer' automatically to other parts of the retina? Will there be similar transfer if, instead of altering the retinal position of the figure, we alter its size or spatial orientation?

These questions, of which an experimental study is described in this paper, are special cases of the much more general problem of how far the reactions of an organism are determined by the excitation of particular receptor cells, and how far by *relationships* or *configurations* within the whole field of stimulation. No attempt will be made here to discuss in any detail the wider implications of this general problem, or the varieties of terminology which have been employed in the experimental literature. In this paper we shall use the term 'transposition' (cf. Koffka, 1935) to mean a change in a complex group of stimuli in which some kind of relationship or configuration is kept constant, although the individual stimuli are altered. In this sense a visual pattern can be 'transposed' to a different part of the retina or to a different size, or a melody 'transposed' to a different key or loudness. If an organism has learnt to respond in a particular way to a fixed pattern of stimuli, and continues to respond in the same way when the pattern has been transposed, this would obviously be an example of 'transfer of training' (cf. Woodworth 1938, ch. 8). If any such transfer can be shown to be due to the constancy of the relationships or configuration, and not due to other factors, we shall refer to it as 'configurational transfer'.

(1) *Previous work on visual transposition*

A number of previous experiments have dealt with the effects of certain forms of visual transposition.

Most of the experiments have been concerned with the problem of 'relative' versus 'absolute' choice. The early work by Köhler (1918), in which he found for instance that an animal trained to prefer a light grey to a medium grey would, in future, prefer a medium grey to a dark grey, has been quoted as a classic example. Köhler experimented with hens, chimpanzees and a human child, and used brightness, size and hue (in separate experiments) as the basis for choice. He claimed that 'relative' choice, while not an invariable rule, was

¹ Paper read at a meeting of the Experimental Psychology Group on 19 October 1946.

more frequent and much better retained than 'absolute' choice. Subsequent workers, using a wide variety of animals and a number of different experimental conditions, have obtained varying results. References to the earlier literature are given by Klüver (1933), who also describes extensive studies of this, and related problems, in monkeys. More recently Spence (1937), working with apes, and Jackson, Stonex, Lane & Dominguez (1938) working with young children, have found that the tendency towards 'absolute' choice increases with the degree of difference between the stimuli, and also with the degree of over-training. They have argued that the conditions under which the tendency to 'absolute' or 'relative' choice will predominate are consistent with hypotheses derived from Pavlov's concepts of 'generalization' and 'differentiation'. A theoretical discussion of the problem is given by Hull (1939).

Evidence of a rather different kind has been given by Fields (1932) and Lashley (1934 and elsewhere) who found that, under suitable conditions, an animal trained to react positively to a given figure will continue to react positively when the brightness relationship between the figure and the background is reversed, or when various other changes are introduced in the figure.

All of these results are difficult to reconcile with any simple theory of neural pathways, or sensori-motor arcs. But they do not provide direct evidence about the effect of repeating the same *retinal* stimulus, since the stimulus object was always exposed for a considerable time while the choice was made, and there was no control or record of eye movements during this period. Uncontrolled eye movements would, of course, make the parts of the retina stimulated different in different trials, and (except in a uniformly illuminated environment) the state of adaptation would also vary. It may well be argued that, without such control, responses to the absolute nature of the stimulus had no fair chance to become established, and that we do not really know very much about the actual sequence of retinal stimuli. With animals or young children, control or recording of eye movements (during the whole of the relevant periods in learning experiments of this kind) has great difficulties; and adult human subjects seem to have been avoided in order to eliminate complications due to verbalization (cf. Jackson *et al.* 1938, p. 581).

(2) *General description of present experiments*

In the present experiments an attempt was made to control the stimuli as accurately as possible. Retinal adaptation was maintained at a reasonably constant level. Fixation of a mark on a dimly illuminated screen was used to control the part of the retina stimulated. The figures were exposed for such a brief period (1/25th sec.—which is below the oculomotor reaction time) as to eliminate complications due to eye-movements.

Adult human subjects were used, and they were first trained to respond 'correctly' (by pressing the appropriate one of a set of four Morse keys) to each of a series of figures. Reaction times and errors could be measured. When the response to each figure had become immediate and (to most of the subjects) almost 'automatic', the critical tests of transposition were introduced. Each figure was then exposed sometimes in its accustomed position, but on other occasions—with no warning to the subject—in another part of the visual field. Similar tests were made with the figures altered in size or spatial orientation but not in retinal position.

The subjects were unaware of the purpose of the experiments, or that any transposition was to be introduced. Both during the initial training and during the critical trials the

order of exposure of the figures was a random one, so that the subject had no means of knowing which stimulus to expect next.

Control experiments were done to measure the extent to which any apparent 'configurational transfer' could be explained by other more general factors such as familiarity with the apparatus and procedure.

II. APPARATUS

Fig. 1 shows the apparatus diagrammatically and in plan. The subject sat with his chin on a rest and his forehead against a headrest *H*. When a 'ready' signal was given the subject looked with his right eye *E* (his left being covered with an eye shield) at a fixation mark *F* on a dimly illuminated ground-glass screen *G*. This screen was 19 in. from the subject's eye, and its illumination was about 0.1 foot-candles.

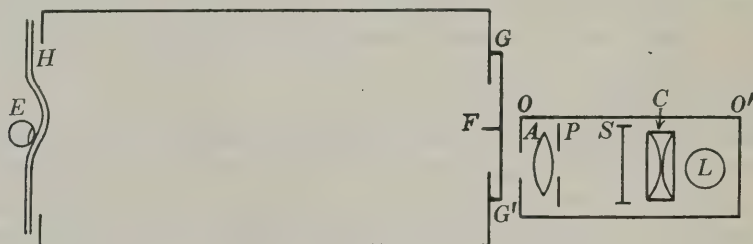


Fig. 1

An optical lantern *OO'* made it possible to throw a figure on to the screen on either side of the fixation point. This lantern consisted of an electric lamp *L*, a condenser *C*, a slide holder *S*, and an anastigmat lens *A*, with which was incorporated a photographic shutter *P* set to give an exposure of $1/25$ sec. The whole of the optical lantern rested on a horizontal sheet of plate glass, and was so arranged that it could be moved silently from one side of the screen *GG'* to the other. A number of tests showed that no one could detect by visual, auditory or other clues the side of the screen on which the lantern was set, i.e. the side on which to expect the next exposure.

Four figures were used (*A*, *B*, *C* and *D* in Fig. 2) in the main experiments. Each figure consisted of four circles of light of equal size, the lantern slide in each case being a sheet of metal with four equal holes drilled in it; hence each figure threw the same total amount of light on to the subject's eye. The size of the figures can be seen from the scale in Fig. 2, the circles being of 0.15 in. diameter, and (as viewed by the subject) of about 150 equivalent foot-candles. The figures were always shown in one of two positions, namely with the centre of the figure exactly 1 in. to the *right* or exactly 1 in. to the *left* of the fixation mark. Under these conditions of brightness, exposure time and retinal position, the figures shown in Fig. 2 (and also the smaller figures used in the experiment on change of size described later) were easily discriminable.

The subject sat with the four fingers of his right hand resting comfortably on four Morse keys. The photographic shutter in the lantern was synchronized with an electric contact so that a Craik chronoscope, reading to $1/100$ th sec., was started at the moment of exposure. A selector switch was set by the experimenter before each exposure so that pressing the 'correct' Morse key stopped the chronoscope, while pressing any other key was registered as an 'error' on an electromagnetic counter.

III. SUBJECTS AND EXPERIMENTAL PROCEDURE

The subjects were 102 R.A.F. aircrew, who were attending the laboratory for other purposes, and who volunteered also to do this experiment. In the instructions they were told that this was a test of reaction times to patterns of light, and the method of exposure and reaction was demonstrated (with slides which were *not* used in the actual experiment). It was explained that they would have to find the 'correct' response to each figure (this could be identified by the sound made in stopping the chronoscope) by trial and error in the first place. The need for accurate fixation when the 'ready' signal had been given was stressed, and the subjects were continually urged to improve their reaction times, while avoiding errors. Their average reaction times were worked out and shown to them at the end of each 'run', i.e. knowledge of results was used as an incentive.

Every subject was given ten training 'runs' on the apparatus, each 'run' consisting of sixteen exposures (four of each plate) in random order. (A 'run' usually took from 5 to 10 min.) Two plates always appeared on the left of the fixation mark, and the other two always on the right. Thus for one subject the figures A and B (in Fig. 2) might be on the

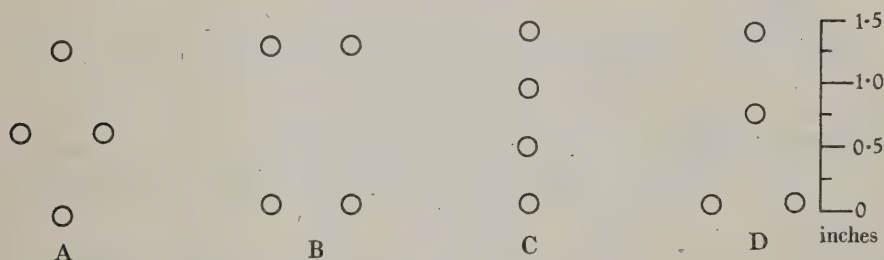


Fig. 2

left, and C and D on the right, A meaning 'first finger', B 'third finger', and so on; while for another subject a different arrangement would be used.

In the transposition experiments the critical tests were introduced in the eleventh and twelfth runs. For instance, in the experiments on the effect of *change of retinal position*, each plate now appeared equally often on the side where it had previously been shown and on the opposite side. With other subjects, upon whom the effect of *change of size* was being tested, each plate was, in half the exposures, replaced by another which was geometrically similar but smaller in size. In this case the linear dimensions, both of the pattern and of the circles of which it was composed, were exactly half those of the original pattern (thus reducing the amount of light falling on the subject's eye to one-quarter). In the tests of the effect of *change of spatial orientation* (with a third group of subjects) the plates were, in half the exposures, rotated through 90° from their original orientation. The subjects were given no warning that there would be any change of conditions in these runs.

The subjects (R.A.F. aircrew) were, of course, not trained in giving 'introspections', in the conventional sense of experimental psychology; moreover it was felt to be undesirable to ask many leading questions until the end of the whole experiment. We therefore contented ourselves with questions such as "How did you get on that time?" and "Do you use any special 'system'?", after the early training runs. Later in the training the subjects were also asked whether the task was now becoming 'automatic'. After the end

of the whole experiment they were more closely questioned about the last two runs (containing the tests of transposition). In fact, the reports made by the subjects after each run were not as long as would have been expected from trained psychologists; and it could not be assumed for instance that if a subject did not *report* 'naming' the figures, he had not done so. On the other hand what *was* reported impressed us as reliable. The subjects were asked not to discuss the experiment with one another and there is every reason to think that this request was strictly carried out.

IV. RESULTS

(1) *Transposition experiments*

(i) *Initial training.* With most of the subjects distributed practice was used during the initial training, the first nine runs being spaced at convenient intervals over a four-day

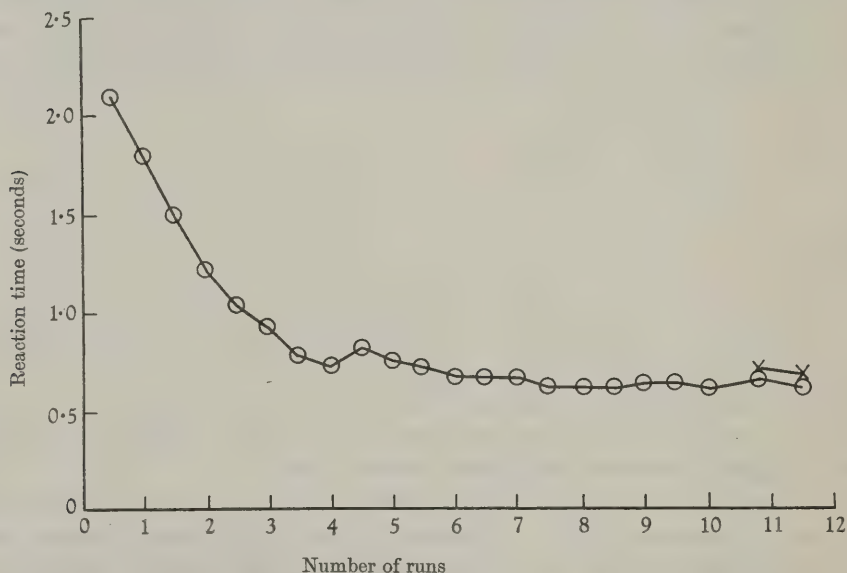


Fig. 3. Change of retinal positions. $\bigcirc$ — $\bigcirc$ Original figures. $\times$ — $\times$ Transposed figures.

period. The last three runs (including the two critical test runs) were taken in immediate succession on the fourth day.

The subjects usually began the first run with an average reaction time (i.e. time before making the *correct* response) of about 2 or 3 sec. This was gradually reduced to about 0.6 sec. after three or four runs, at which average value it remained fairly constant until the end of the tenth run. The form of the learning curve can be seen from Fig. 3, which shows average reaction times for successive series of eight exposures (i.e. there are two points per run). The curve is for 34 subjects in the first transposition experiment described below, and each point therefore represents the mean of 262 reaction times. Similar learning curves were obtained for the groups of subjects in the other experiments.

At the beginning of the first run the number of errors was, of course, purely a matter of chance (i.e. an average of about 1.5 per exposure) but, as the subject learnt the task, this was reduced to an average of about one error in fifteen exposures after three or four runs,

and remained constant at that value. These residual errors appeared to be due to carelessness.

Many of the subjects reported, after the first or second run, that they were giving names such as "square", "diamond", "line", and "T" to the figures, and memorizing the correct responses by phrases such as "square two", "line three", etc. Later in the learning some of the subjects reported that they still used these mnemonics, but that they often found that the correct finger had already responded (before verbalising had occurred). Others reported that verbalization dropped out as the task became automatic. A few said that during the learning they had consciously used the position of the figure in the visual field as a guide.

With 8 subjects massed practice was used, instead of distributed practice. The whole twelve runs were taken in immediate succession, apart from a break of about 15 min. between the sixth and seventh. The subjects showed no sign of fatigue or boredom, and the initial learning curve was quite as rapid as that for distributed practice. The effect of transposition also was similar to that for the other subjects, and the results have therefore been treated together.

(ii) *Change of retinal position.* Forty-four subjects were used in this experiment (36 with distributed practice and 8 with massed practice). When the 'transposed' figures were shown the subjects varied considerably in their behaviour.

Three subjects did not at first respond with the Morse keys, but protested that the experimenter had made a mistake. They were encouraged to proceed, and finally mastered the transposed figures satisfactorily; but their results have been omitted from the average figures for reaction time owing to their initial failure to respond.

Eight subjects noticed the transposition at once, and afterwards said that the task was now much more difficult and confusing. These eight subjects all showed a considerable increase in reaction times, but no significant increase in errors. There was some indication from their reports and answers to questions at the end of the experiment that many, if not all, of these subjects (as well as the three previously mentioned) had consciously used the *spatial position* of the figures in the initial learning, e.g. they had learnt that a figure on the right always meant either first or third finger, and they had then learnt which figure corresponded to each of these responses—and similarly for the pair on the left. (The remaining subjects, whose results are discussed below, very seldom reported that they had learnt in this way.)

Fourteen other subjects noticed the transposition at once, but said that it made no difference to the difficulty of the task. These subjects showed only a small increase in reaction time, and this was not statistically significant.

The remaining 19 subjects *did not notice* the transposition at all. Even when so closely questioned at the end of the whole experiment that they suspected that *something* had been altered in the last two runs, some of them could only make suggestions such as "Well, perhaps you altered the length of the exposure a bit". Others guessed that the plates *might* have sometimes appeared on the wrong side, but insisted that they had not noticed this at the time. With this group of 19 subjects there was no significant increase in reaction time; in fact their average reaction time for the first transposed plate was slightly (but not significantly) less than their average reaction time for the last two training runs.

Fig. 3 shows for the 34 subjects who were trained by distributed practice (and who did

not fail to respond when the transposition was introduced), the average reaction times to both the transposed and untransposed figures in the critical test runs, as well as the course of the original learning. It will be seen that, although this group included 6 subjects who were disturbed by the transposition, the average effect on reaction time was quite small.

(iii) *Change of size.* In this experiment (with another 12 subjects) the tests of transposition consisted of the substitution of figures of half the linear dimensions of the originals, with no change of position of the centre of each figure. Otherwise the conditions were as before, distributed practice being used in the original training. All the subjects noticed the transposition and one at first failed to respond. A few thought that the task was now more difficult, but most of them said that the change made little difference. There was a slight average increase of reaction time which was just statistically significant.

(iv) *Change of spatial orientation.* In this case the transposition consisted of rotating each plate through 90° about its centre. All the 12 subjects who took part noticed the transposition at once, and most of them were considerably disturbed by it. One failed to respond at first. There was an average increase of reaction time of 83% for the first transposed plate, and of 45% for the whole of the eight transposed plates in the first test run. These figures are statistically highly significant. There was no marked tendency to press the wrong key, i.e. no marked increase in errors.

(v) *Combinations of changes.* With another 12 subjects various combinations of the above types of transposition were used, e.g. the plates might be altered both in retinal position and in orientation. In general the effect seemed to be (as might be expected) that a combination of changes produced more disturbance than a single type of change. The number of subjects used for any particular combination of changes was too small for statistical treatment of the results. It is, however, of interest that there was *one* subject who, *when given all three types of transposition simultaneously* (i.e. when the figures appeared on the opposite side of the retina, at half their original size, and rotated through 90°), showed no appreciable disturbance in his reaction times or error scores. He noticed the transposition—as is hardly surprising—but said that it did not upset him.

(2) *Control experiments*

(i) *Change of figures.* It is obvious that *some*, at least, of the apparent 'configurational transfer' found in the transposition experiments just described may well have been due to other factors such as increasing familiarity with the apparatus, greater facility with the Morse keys, or improved methods of learning a task of this kind (cf. Woodworth, 1938, ch. 8). In order to form some estimate of the degree to which these more general factors were acting we therefore did an experiment in which the subjects, after the normal initial training, were required to re-learn the task when four entirely new figures had been substituted for the originals.

The initial training was exactly as before, and with the plates shown in Fig. 2. At the beginning of the eleventh run the subjects were told that new figures would now be used, and that they would have to learn the 'correct' responses just as in the previous learning. The four figures which were substituted for the originals are not shown in Fig. 2, but, like those, they consisted of patterns of dots which could easily be discriminated.

There were 12 subjects, whose average reaction times during the initial learning, and after the substitution of the new figures, are shown in Fig. 4. It will be seen that there is evidence of a considerable amount of transfer of training under these conditions, since the 're-learning' curve both starts much lower than the original curve and also falls much more rapidly. On the other hand, the disruption of the curve with the change to new figures is in marked contrast to the relatively slight effects observed in the transposition experiments.

When the new plates were introduced the *error scores*, of course, rose to their original high value (since the subjects had no means of predicting the correct responses to the new figures), but in errors, as well as reaction times, the re-learning was much faster than the original learning.

(ii) *Change of response.* In another rather similar control experiment the change introduced was in the responses to be made, rather than in the figures shown.

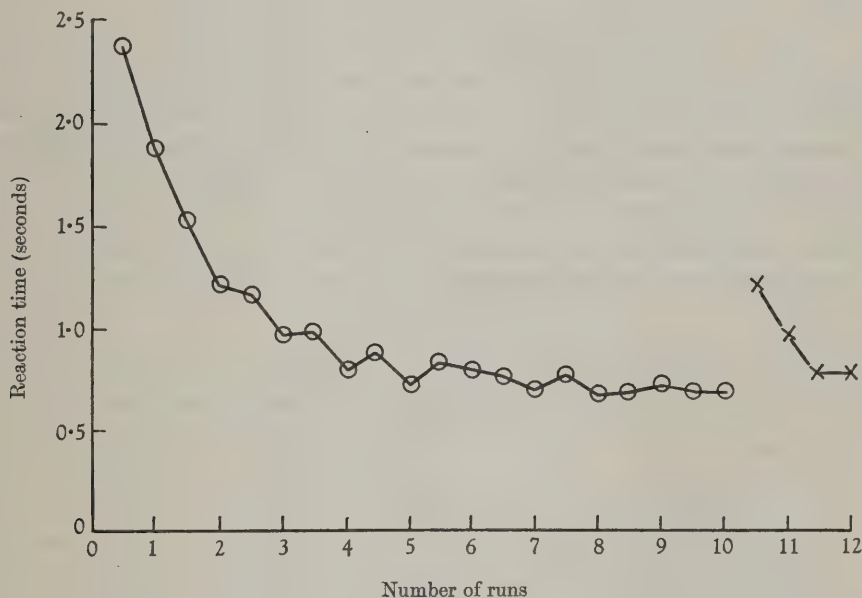


Fig. 4. Change of figures. $\circ$ — $\circ$ Original figures. $\times$ — $\times$ New figures.

There were 12 subjects and the original plates were used throughout, the initial training being exactly as before. At the beginning of the eleventh run the subjects were told that the 'correct' response to each figure might now be different from what it had been, and that they would have to find the new responses by trial and error.

Under these circumstances we might well have expected that 'interference' with the old habits would play an important part. In fact the amount of disruption of the curve (in terms of reaction times and errors), and also the rate of 're-learning', were almost identical with those in the experiment on change of figures which has just been described (see Table 1).

(iii) *Learning by retinal positions.* It will be remembered that in the transposition experiments some of the subjects behaved as though an alteration in the position of a figure from one side of the retina to the other had no effect at all, i.e. as though the same

pattern, whatever its retinal position, always excited an identical central mechanism. These subjects did not even know when the retinal position was changed. Other subjects, however, who had learnt in terms of position were aware of—and often disturbed by—such a change.

As a check on the ease with which the retinal position can be used in learning of this kind, we therefore did an experiment in which position was a *necessary* clue to the correct response. In this case the subjects saw only two of the original plates, but each of these required a different response according to whether it was on the left or on the right. Thus there were only two figures, but there were still four correct responses.

Learning under these circumstances presented no additional difficulties to any of the four subjects employed; it was in fact somewhat more rapid than the original learning in any of the previous experiments.

No transposition of any kind was introduced.

(3) *Summary of quantitative results*

It has already been pointed out that at the end of the initial training (i.e. after ten runs) in all the experiments the subjects had reached an average reaction time of the order of 0.6 sec., and an average error score of about one in every fifteen exposures. (There were, of course, individual differences between subjects, of the kind to be expected in any reaction time experiment.)

The effects of the various types of transposition, and of the other changes introduced in the control experiments, may be compared from the data given in Table 1. The average

Table 1. *Effects of transposition and other changes on reaction times*

Nature of change	No. of subjects*	Increase in reaction time	
		1st eight plates (%)	1st plate (%)
(1) Transposition:			
(i) Retinal Position:			
(a) Whole group	44 (3)	(13)	(17)
(b) Disturbed by change	8	26	70
(c) Noticed change	14	(16)	(16)
(d) Unaware of change	19	(6)	(-2)
(ii) Size	12 (1)	21	15
(iii) Spatial orientation	12 (1)	45	83
(2) Control Experiments:			
(i) New figures	12	73	163
(ii) New responses	12	92	174

* The figures in brackets in this column show the number of subjects who at first failed to respond at all under the new conditions, and who are, therefore, omitted from the calculations of average reaction times (see text).

In the last two columns figures in brackets indicate that these changes of average reaction times were not statistically significant.

reaction time of each group in the last two training runs (i.e. the ninth and tenth) has been taken as a basis, and the percentage increase in reaction time has been calculated for the first *eight* plates shown under the new conditions (e.g. first eight transposed plates), and also for the very first plate shown under the new conditions. Changes which are not statistically significant are shown in brackets. Figures for errors are not given, since, except in the case of the control experiments with new plates or new responses, the number of errors was so small that no change was statistically significant.

In interpreting the table it should be remembered that if there were *no transfer of any kind* (i.e. if the initial training had no effect on behaviour under the new conditions) the increase of reaction time to be expected would be a return to the original value of about 2 or 3 sec. This would be an increase of about 300%. Even in the control experiments with new plates or new responses the observed increase was less than this, so that we can infer some positive transfer in all cases. The relatively small increases in the transposition experiments are therefore an index of the extent of configurational transfer. (On quantitative methods of expressing degrees of transfer cf. Woodworth, *loc. cit.*)

V. DISCUSSION

If the results described in this paper were not due to any artefact—i.e. assuming that the relevant conditions were adequately controlled—some of the experiments show a very high degree of ‘configurational transfer’. Moreover these experiments, unlike those referred to in the introduction, were such that they might have been expected to favour the formation of fixed pathways rather than the occurrence of configurational transfer.

The most striking effect was that of change of position, where, with about half the subjects, configurational transfer was almost complete as measured by objective standards of time and error, and many of the subjects were quite unaware of the transposition. It is of interest that change of size and change of orientation produced more disturbance: but in both cases the degree of configurational transfer was very marked. The first two of the control experiments seemed to show that these results were not entirely due to the operation of more general forms of transfer, but were genuine configurational effects.

The reasons for the large differences in degree of transfer between individual subjects, under identical objective conditions, are not very clear. There was however some indication, in the case of change of retinal position, that those subjects who had consciously used position as a clue during the initial training were most affected by the transposition. The third control experiment shows the ease with which position (rather than shape) can be caused to play a primary part in learning of this kind. The subjects who turned out to be most affected by the transposition showed, objectively, an initial learning curve and a level of final performance which were similar to those of the other subjects; and this whole phenomenon may be an example of habits which appear objectively identical (by present methods of measurement), but which really differ in the nature of their underlying mechanism if they were originally acquired in different ways.

Our experiments do not suggest any clear picture of the *mechanism* of configurational transfer. Like the earlier experiments, quoted in the introduction to this paper, they are difficult to reconcile with any simple theory of neural pathways, even if we assume, with Hilgard & Marquis (1940, pp. 333–4) that “equivalent stimuli which activate different groups of afferent neurons presumably have some central conduction pathway in common, and the learning modification would occur in that part of the pathway which is common to both stimuli”. In the case of configurational transfer it is not easy to imagine a central common pathway which would explain the facts. But, having pointed out the difficulty of this type of theory, it is still possible to agree with the conclusion of Hilgard & Marquis (*loc. cit.*) that “no alternative to the theory of modification of synaptic resistance has ever been stated in a form specific enough to permit critical evaluation”.

The need for further experiments to determine more fully the conditions and origin of

the phenomena of configurational transfer is obvious. Speculation on the underlying mechanism is of little value unless it can be made definite enough either to explain the known facts or to predict hitherto unobserved phenomena. It would, however, be of interest to determine whether configurational transfer effects of the kind described in this paper obey laws analogous to those of the transfer effects described by Köhler, Spence and Jackson *et al.* (cf. section I), i.e. to determine how the present transfer effects behave in respect of *retention*, how they vary with the degree of *absolute* differences introduced in the transposition, and how they vary with the degree of *over-training*. More fundamental, in any theory of behaviour, is the problem of how far the tendency to react to configurations in an innate quality (as has been suggested by the *Gestalt* school) and how far it is (in Pavlov's sense) a very 'generalized' result of past experience. The experimental evidence, so far, does not give a conclusive answer.

Finally, it should be added that the fact that many of the subjects in the present experiments used verbal methods of learning (e.g. 'naming' the figures) does not seem in any way to help to solve the problem of configurational transfer. Naming was undoubtedly an important part of the learning process with many subjects, and in some cases it continued to be explicit during the transposition tests. In these cases the connexion of a particular name with a particular figure and with the appropriate finger response was a part of the learning process. The problem of configurational transfer could then be stated in this way: "Why is it that a name which has been attached to one pattern of stimulation during the learning is now immediately aroused when the pattern is transposed?"—which is very similar to the problem when *no* naming is involved. The same would be true of arguments in terms of 'meaning' or 'recognition of shape'. Hull's critical discussion of "the role of 'intervening variables' in behaviour theory" (Hull, 1943, p. 21) is relevant here.

VI. SUMMARY AND CONCLUSION

1. A number of adult human subjects were trained to respond immediately, by pressing appropriate Morse keys, to each of a series of simple figures, which were exposed momentarily at fixed positions in the visual field. Tests were then made of the degree of 'transfer' when the figures were altered in retinal position, size, or spatial orientation.

2. It was found that transfer was greatest in the case of change of position. Many of the subjects showed no significant increase in reaction time or errors when a figure was 'transposed' from one side of the retina to the other; in fact they were often unaware that any change had been made.

3. Transfer was less complete when the figures were altered in size or spatial orientation, but was still very marked.

4. Control experiments suggested that these results were genuine cases of transfer due to the constancy of the configuration.

5. The relationship of the results to those of other experiments on visual 'transposition' is briefly discussed.

We wish to acknowledge our indebtedness to Prof. F. C. Bartlett for many suggestions during the course of the work, to the late Dr K. J. W. Craik for help in the construction of the apparatus, to the many R.A.F. aircrew who acted as subjects, and to the Rockefeller Foundation for financial aid.

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PUBLICATIONS RECENTLY RECEIVED

Intelligence and Fertility. By SIR CYRIL BURT. London: Eugenics Society and Hamish Hamilton Medical Books. 1946. Pp. 43. 2s. net.

Sir Cyril Burt produces evidence to show that in general the children of larger families have lower I.Q.'s than the children of smaller families, and from this he deduces that there has been a 'decline' in the average intelligence of the country during the last generation. It may be thought unfortunate that he has used the word 'decline', since it suggests an active deterioration of some sort, whereas there is no evidence at all to show that any individual has become less intelligent or even that any parents of a certain status and producing a family of a certain size have children who are less intelligent than those from similar parents a generation ago. What he has shown in point of fact is that, given the premises (a) that the children of larger families are less intelligent than the children of smaller families, and (b) that the number of larger families increases relative to the number of smaller families, then it follows that the average intelligence of all children will decrease as long as (b) holds—which is a rather obvious arithmetical consequence.

Sir Cyril does not, however, produce evidence of the truth of (b). Presumably research into the census returns would give the relative frequency distribution of number in family for successive decades and determine the truth or not of this point. However, Sir Cyril does not do this. Instead, he takes a group of tested children and draws up a table showing the relationship between I.Q. level, percentage of children reaching each level and average numbers in the families of these children. (Table II, p. 20.) The average I.Q. of all these children—the total number is unspecified—is 100.0. He then proceeds to calculate what *would be* the average I.Q. of these children if the families of different sizes were proportionally represented. This average I.Q. would be 101.9. Hence he concludes that there has been a loss of 1.9 I.Q. points. But this 'loss' is merely the difference between the average I.Q. of an actual population and that of a mythical population. If the mythical population had existed at a previous date, this loss could have been regarded as a real one. Since, however, there is no evidence that it did exist, the 'decline' in average intelligence can only be regarded as a fiction. (Incidentally, it is difficult to understand how a correlation between intelligence and family size of only -0.24 can yield the second and fourth columns of Table II.) This method of indirect estimation is followed by a direct estimate of the 'decline'. By calculating I.Q.'s from "certain of the best of the Binet tests", which were given twice to London schools, Sir Cyril finds an average rate of decline of 0.87 I.Q. points in 20 years. Also, between 1912 and 1932, it was estimated, "as far as the estimates can be trusted", that the number of children aged 10–11 who had an I.Q. over 125 dropped from 5.93 to 5.12%, whilst those with an I.Q. below 70 rose from 1.54 to 1.71%. This evidence is by no means convincing. Sir Cyril himself says "it would be somewhat rash to accept such estimated changes as due to differential fertility" and instances such factors as migration of families to and from London. Apart from this there is another factor—the unknown effects of a world war in the years under review. Children of 11 in 1912 had grown up in a period of prosperity; those in 1932 had spent their young days in the lean years following the war, and there is evidence that intelligence is associated with standard of nutrition.

In Table III, p. 32, Sir Cyril gives a gloomy estimate of the level of the national I.Q. at the end of the century, obtained by extrapolating the previously estimated rate of decline. For this to have any validity at all, it must be assumed that his present estimates are correct and that the present differential birthrate continues. Even if that were so, the question arises, which is the more important, the percentage of highly intelligent people or the gross number of such? If the former is regarded as the more important, the percentage could readily be increased by extending the principle of euthanasia to all children with an I.Q. below 70; but this would in no way affect the gross number of intelligent people available as a "national resource of mind-power".

It is reasonable to speculate upon the results which would have been obtained had the investigation been made in Victorian times, when the Forsytes were as prolific as were the fishmongers. Or one might go back to the Middle Ages, when for many generations the possessors of the highest I.Q.'s were mainly the celibate inmates of religious institutions. On Sir Cyril's argument there must have been an enormous decline in the average intelligence of the nation since those times.

Since the whole thesis depends on the use of I.Q.'s, certain questions arise. In the first place, on what children have the intelligence tests used been 'standardized'? If intelligence is related to size of family, norms will obviously be affected considerably by the type of population on which they are established. For instance, is an exact balance held between the relative proportions of families of different size, or is

the sample overweighted by one class or another? Again, in calculating the correlation between intelligence and size of family, is the result weighted by the inclusion of more than one member of the larger families? For example, there are six dull Smith brothers in a school and one bright Robinson boy, who is an only child: does the Smith family appear six times in the correlation table? If so, the correlation coefficient is to some extent spurious. It would also be interesting to know, when considering the average I.Q.'s of the 'best' and the 'poorest' schools in a borough, if the standard of teaching is the same quality in both schools.

The subtitle of this paper is: "The Effect of the Differential Birthrate on Inborn Mental Characteristics". Some readers may regard this as question-begging. Surely the only deduction which may legitimately be made from the data presented is that *intelligence* has an effect on the *birthrate*, in the sense that more intelligent people at present tend to produce smaller families than do less intelligent people. Such a thesis would be readily understandable and the reasons for such a state of affairs not difficult to determine. One obvious reason, for example, is that a professional man has such a lengthy period of training that he is frequently not sufficiently established in his career to contemplate marriage before the age of thirty, whereas the 'working man' may be fully established at twenty, and there is a direct physiological relationship between age of marriage and fertility.

Turning finally to the Appendix to this paper, we find that Sir Cyril attempts to define what is meant by intelligence and to decide what part of it is innate and what part is accounted for by environmental factors. He suggests as definition that intelligence is "innate, general cognitive ability". Later, on p. 41, he says: "Every psychologist admits that the I.Q.'s furnished by intelligence tests...may be influenced in *some* degree by schooling and other environmental differences." Finally he is of the opinion that most psychologists would agree with Barbara Burks that "home environment contributes about 17% of the variance in I.Q. (as actually tested); parental intelligence accounts for about 33%; and the total contribution of innate and heritable factors is probably not far from 75 or 80%". These figures appear to be the result of a statistical study, which might be open to criticism, but if we accept them, the interesting question arises, what is the nature and relative importance of the small percentage not accounted for? -

E.G.C.

Human Factors in Air Transport Design. By ROSS A. MCFARLAND. New York and London: McGraw Hill Book Co., Ltd. 1946. Pp. xix+670. \$6.00.

Many people in this country have for a long time been looking forward to the day when Dr McFarland would be able to publish the results of his own research, and of his prolonged studies of the work of others, on the design of aircraft and air transport conditions with due regard for the physiology and psychology of crews and passengers. The book, now that it has arrived, will be found of immense service in many ways. The war showed the enormous importance of what was to be called 'personnel research' for ensuring success and safeguarding unnecessary peril in the air. But the gain for knowledge and practice does not seem to have been passed on properly to the great civil air corporations, to the general public, or even to most physiologists and psychologists. The engineer still has, as a rule, a wholly inadequate conception of the importance for him of various aspects of biological science, and the same is true of most of the other scientists who are engaged upon physical developments. McFarland's book is authoritative, thoroughly well backed up by wide and recent studies as well as by much original research into problems of high altitude flight, and it covers a wide field. In thirteen chapters a mass of work is described covering the effects upon human physiological and psychological activities of very nearly all the leading environmental conditions. There are special studies of the effects of altitude; ventilation, temperature and humidity; of air-carried infection; of a variety of harmful gases; of noise and vibration and of acceleration and motion generally. Some discussion is offered of problems of the internal design of aircraft instruments so as to facilitate their accurate manipulation, and some to passenger comfort and accommodation. A long chapter deals in detail with accident prevention.

The whole field of aircraft and air transport design is one which is in such rapid change and development that it would be practically impossible for any book to achieve publication which did not already in some respects seem to lag a bit behind achievement. Much work has been done in the physiology and psychology of flight, which cannot find a place in this book. But the publication will be of lasting interest even when, perhaps, a large part of its reported results have become out of date. For in this book Dr McFarland has shown very clearly and beyond dispute how, in this great field of modern enterprise, the physical and biological scientist must freely co-operate. Every psychological laboratory should possess a well-studied copy of this book.

The Health of the Industrial Worker in India. By T. BEDFORD. A Report issued by the Department of Health of the Government of India. 1946. Pp. 34.

This pamphlet contains a report of a three-month tour of inspection and observation in some of the industrial areas of India. The conditions in which Indian workers live and work, as described by Dr Bedford, leave a very great deal to be desired. In almost every branch of the study of industrial health India appears to lag far behind Western nations, and Dr Bedford gives an expert and comprehensive survey of the fields of research which are essential to the health, efficiency and welfare of the Indian worker.

Forecasting College Achievement. Part I: General Considerations in the Measurement of Academic Promise. By ALBERT BEECHER CRAWFORD and PAUL SYLVESTER BURNHAM. Yale University Press (London: Geoffrey Cumberlege, Oxford University Press). 1946. Pp. xxi+291. 25s. net.

This book is the first of three on the theory and practice of mental measurement, with special reference to tests for higher education. The authors draw distinctions between tests of general intelligence and of more specific abilities, including under the latter ('backward-looking') tests of achievement, ('more forward-looking') tests of aptitude and somewhat hypothetical unitary traits.

Perhaps the most valuable function performed by Drs Crawford and Burnham is their description of the difficulties encountered, though often unrecognized, by the vocational and educational psychologist, and their stringent criticisms of many of the methods which he uses and abuses. The book has also a more positive aspect: several batteries of American University and Service tests are described and discussed, the potentialities of 'differential' psychological tests are illustrated and explanations are given of the statistical techniques most commonly used in 'psychometrics'. The authors criticise that type of test 'validation' which is purely 'internal', i.e. strictly a measure of consistency rather than validity, and outline the essentials of reliable external criteria and the difficulties inherent in obtaining them. They stress the dependence of all statistical findings on the particular set of data on which the statistics are based: test results "represent measurement by inference from a limited sampling of tasks, representing a limited number of functions, appraised by a limited number of items, as administered to a limited number of people". They point out the fact that what were originally means to an end have, for many statistically minded psychologists, become ends; that, for instance, 'Objectivity' in testing and in test interpretation often becomes an end in itself, the psychologists concerned forgetting that "Objectivity in scoring . . . does not necessarily offset or eliminate subjectivity in the construction" (of tests). In this connexion they also discuss factorial analysis, commenting that with this technique "subjectivity, or predilection of the investigator, enters at the crucial ultimate stage". The chapter on 'Unitary Traits and Primary Abilities' contains a fair but critical exposition of the work of the factorial analysts, with special reference to Thurstone's centroid method. In addition to the fundamental subjectivity of the method referred to above, the criticisms include the following: that the terminology is at times misleading (e.g. Thurstone's use of 'memory'); that it is not always possible to tell which factors are really 'primary' (e.g. children's test scores tend to yield more 'g' than do adults); that the statistical techniques involved are in some respects more refined and in others less refined than is warranted; that the tests used are often incautiously designed (e.g. short time limits producing a spurious common factor—of speed—between tests) and inadequately validated.

The book contains several rather muddled discussions on the relative merits of restricted answer and essay form questions. Drs Crawford and Burnham rightly point out that whilst the former are, in certain senses, more objective than the latter, and more appropriate if a great number of items are to be covered, they are a less suitable instrument for probing deeply into the subject's knowledge. The authors do not draw any clear distinction between the various *types* of restricted answer. Their final verdict should, however, please both the "subject matter experts who have a limited grasp of testing methods" and the "testing experts who have a limited grasp of subject matter"; they advocate that for practical purposes the two methods should be combined.

In the chapter entitled 'A Sample Attitude Battery', the Yale Battery of "prognostic and differential tests" is explained. These tests are described as occupying "a middle ground between rather narrowly prescribed achievement indices and measures of intellectual 'factors'" and a comparison is made between profiles (group and individual) obtained from these seven tests and success in college career. A good measure of agreement is claimed to exist between these profiles, properly interpreted, and success in one of the three groups of faculties: "liberal arts study", "pure science and mathematics" and "applied

science such as engineering". The agreement is in general quite impressive, but examination of the examples of the tests, given in the Appendix, suggests that some of them may well not be testing what they are intended to test. The "Artificial Language" test, for instance, appears *prima facie* to assess elementary reasoning plus a high degree of accuracy, rather than the complex qualities which go to make up a gifted linguist. Inspection of the correlation table on p. 161 confirms this suspicion; "Artificial Language" correlates more highly with Physics grades, for example, than with grades of English and History.

In the last chapter are stated the conditions which a satisfactory test should fulfil, and the methods by which this end may be attained. It contains also a discussion of the measurement and persistence of 'idiosyncrasies', the latter signifying in this context marked differences between different abilities in the same individual.

Parts II and III will cover respectively the measurement of more specifically educational aptitudes and of professional (graduate) abilities; the last part will deal also with attempts to measure interests and personality.

A.W.H.

Personal Adjustment. By KNIGHT DUNLAP. New York and London: McGraw-Hill Book Co., Inc. 1946. Pp. xii + 446. 20s. net.

As a popular and non-technical treatise on mental hygiene, this book lacks integration and logical development. It begins with an elementary discussion of learning and studying. This is followed by a rather superficial discussion of mental disorder and its symptoms, and a brief chapter on "Goals and their Attainments". A discussion of psychotherapeutic methods is confined mainly to 'talking it out' and to environmental modification; but also describes the author's system of the 'negative practice' of undesirable habits such as stammering, i.e. the voluntary repetition of the action, under guidance from the psychotherapist, with intent to eliminate it. This would appear to be valuable for the therapy of certain habits such as worrying if combined with some more satisfactory 'deep' therapeutic treatment. But Prof. Knight Dunlap abhors anything to which the name psychoanalysis could be given.

The latter part of the book is devoted mainly to a discussion of sexual and marital adjustment, and of the upbringing of children, which is clear and sensible.

The Juvenile Courts. By F. T. GILES. London: George Allen and Unwin, Ltd. 1946. Pp. 131. 6s. net.

This book, the author of which was for many years the Chief Clerk of a Juvenile Court, can be thoroughly recommended as an exceedingly readable account of the work of the Juvenile Courts. It shows first how misleading conclusions drawn from the increase in numbers of cases referred to the Juvenile Courts may be avoided. It makes thoroughly clear the distinction between the legal procedure of the Courts when concerned with criminal cases, and when dealing with children not charged with such offences who are brought to the Court as "in need of care and protection" or as being "beyond parental control". Thus in the first case there is a guarantee of the preservation of the 'liberty of the subject' in criminal actions from an encroachment of the powers of the executive Government. At the same time, every endeavour is made to ensure that the child obtains the treatment, after sentence, which is most suitable for his particular circumstances. In this connexion, however, an extension of the probation and psychological services is justifiably advocated, and also an improvement in hostels and approved schools. The description of the psychologist's contribution to the understanding of the child is perhaps a little sketchy; but much technical detail would of course be out of place in a book intended for popular reading.

Children of the Cumberland. By CLAUDIA LEWIS. New York: Columbia University Press (London: Geoffrey Cumberlege, Oxford University Press). 1946. Pp. xviii + 217. 15s. 6d. net.

This book consists in the main of an attractively written study of nursery school children, their homes and families, living in a secluded mountain district of Tennessee. The children are quiet, shy and in the main contented—very different from the active, restless, imaginative children of the Harriet Johnson nursery school in New York's 'Greenwich Village'. The country children, though brought up in great poverty, have an accepted place and a status regulated by long-standing custom in the life and activities of their large families. Yet the fact that this existence makes little demands on them, seems to mean that they fail

to acquire adjustment to the harder demands of a wider life such as is experienced by the New York children; as adults the former remain shy, secluded, often frustrated and unable to cope with anything beyond their narrow and poverty-stricken existence.

The book is a pleasantly written, if perhaps slightly repetitive, contribution to the raw material upon which to base an understanding of the effect upon children's personalities of their social environment. The photographic illustrations are charming.

Why Pupils Fail in Reading. By HELEN MANSFIELD ROBINSON, Chicago: University of Chicago Press. (Cambridge University Press.) 1946. Pp. xiii + 257. 16s. 6d. net.

In her study of reading disabilities, the author's first step was to analyse previous literature on the subject, and to group the apparent causes of the disabilities into visual, neurological, auditory and speech, physical, intellectual, emotional and social and environmental. Then, thirty-seven severely retarded readers, with ages ranging from 6 to 15 and Binet I.Q.'s from 85 to 137, were examined intensively by a group of specialists, psychological and medical. The findings were examined, discussed and evaluated by the group for each case individually. Remedial programmes were drawn up and applied for twenty-two cases, in order to secure further evidence as to the importance of the causes diagnosed.

It was concluded that causation was frequently multiple, and that the more numerous the anomalies belonging to the above categories, the more serious the retardation. At the same time, not all the anomalies detected seemed to have any direct relationship to the reading disability; and some which were originally thought to be important were shown by the results of the remedial treatment not to be so. Emotional maladjustments closely related to family problems were found to be very important causes, but little effect of poor teaching in school could be detected. Visual anomalies, especially of binocular co-ordination, were contributing causes in about 50% of cases. But no study seems to have been made of backwardness in perceptual maturation—the factor discovered by Frank and stressed by Schonell (whose work is not mentioned here). Speech and functional auditory defects (poor auditory memory) were contributory causes in about 18% of cases. Evidence as to inherent neurological 'alexia' was inconclusive. Physical and intellectual disabilities were infrequent.

Although the conclusion that numerous causes contribute to produce reading disability seems valid, this study does little to further their diagnosis, since it provides no fundamental psychological analysis of the causes and their interrelationship.

Alfred Adler: Apostle of Freedom. By PHYLLIS BOTTOME. Second Edition. London: Faber and Faber, Ltd. 1946. Pp. 279. 12s. 6d. net.

BRITISH PSYCHOLOGICAL SOCIETY

REVENUE ACCOUNT AND BALANCE SHEET AS AT 31 DECEMBER 1946

Revenue Account for the year ended 31 December 1946

1945		EXPENDITURE		INCOME	
£	s. d.	£	s. d.	£	s. d.
1945				1945	
93	4 0	To Rent	93 4 0	1862	11 6
37	5 0	Rates	37 2 6		
30	19 3	Lighting, Heating and Cleaning	38 10 4	By Subscriptions Received	...
322	8 6	Salaries, State Insurance and Staff- Annuity Fund	741 8 2	Transfer Fees:	...
179	10 2	Printing, Stationery and Duplicating	453 10 6	Fellows	...
81	18 6	Printing copies of Memorandum and Articles	53 10 0	Associates	...
8	16 6	Expenses of Meetings	19 4 2	Journal Subscriptions—Australian Branch	26 5 0
133	16 8	Postages and Telephone	186 8 5	Interest on Investments	42 5 0
7	6 9	Insurance	8 9 3	Amount received in respect of Insurance claim for Office Furniture, Fittings and Supplies lost in Fire	173 1 11
15	15 0	Audit Fee and Accountancy	31 10 0	Balance, being Excess of Expenditure over Income	500 0 0
31	18 11	Teas	21 2 8		81 12 0
12	1 6	Subscriptions to Outside Associations	7 19 6		
13	12 2	Sundry Expenses	40 19 7		
		Journals, Publication Expenses and Reserves:			
		283 5 0 General	328 15 0		
		118 17 6 Medical	136 10 0		
		171 7 6 Educational	173 10 0		
573	10 0	Annual General Meeting 1946	638 15 0		
46	4 10	Midland Branch Expenses	46 4 2		
1	6 4	Northern Branch Expenses	2 18 8		
-	-	Scottish Branch Expenses	9 13 6		
9	4 6	Committee of Professional Psycho- logists (Mental Health)	11 5 0		
32	7 2	Travelling Expenses	66 2 1		
23	3 6	Provisional National Council for Mental Health—Contribution to Guarantee Fund	- - -		
-	-	Bank Charges and Cheque Books	20 0 0		
1	0 0	Legal Expenses	7 14 5		
5	5 0	Book value at 1/1/1946 of Office Furniture and Fittings lost in fire	- - -		
-	-	Depreciation:	162 0 0		
		Library	47 3 11		
		Office Furniture and Fittings	32 18 7		
67	1 9		80 2 6		
£2227	16 0		£2777 14 5		
				£2227	16 0
					£2777 14

